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U.S. EFFORTS TO COMMERCIALIZE SUPERCONDUCTIVITY

HEARING

BEFORE THE

COMMITTEE ON GOVERNMENTAL AFFAIRS UNITED STATES SENATE

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U.S. EFFORTS TO COMMERCIALIZE SUPERCONDUCTIVITY

TUESDAY, JUNE 28, 1988

U.S. SENATE,
COMMITTEE ON GOVERNMENTAL AFFAIRS,
Washington, DC.

The Committee met, pursuant to notice, at 9:35 a.m., in room SD-342, Dirksen Senate Office Building, Hon. John Glenn, Chairman of the Committee, presiding.

Present: Senators Glenn, Chiles, Pryor, Bingaman, and Roth.

OPENING STATEMENT OF CHAIRMAN GLENN

Chairman GLENN. The hearing will be in order.

The subject of our hearing this morning is superconductivity, one of the newest areas of scientific research. Today I am releasing a major, new Office of Technology Assessment report that evaluates U.S. efforts to commercialize high-temperature superconductivity technologies.

One of the major findings of the report is that, "American companies may already have begun to fall behind," their foreign competitors in the race to commercialize new products and processes based on high-temperature superconductivity.

The last 2 years have witnessed the rapid discovery of new materials capable of conducting electricity without resistance and therefore without heat losses at temperatures higher than scientists previously anticipated. These discoveries hold the promise of amazing new products: levitated trains which speed along at 300 miles per hour, remarkably efficient energy storage systems, supercomputers in desk-top models, and super-sensitive medical imaging equipment cheap enough for any clinic to afford.

But all of this is old—and good—news. The report I am releasing today contains something more—a truly disturbing new finding. OTA's assessment suggests that competitors like Japan are probably already ahead of us in preparing to bring revolutionary high-temperature superconductivity-based products and processes to market. As with semiconductors, video cassette recorders, high-density television, and electronic consumer goods—the list is embarrassingly long—the United States again appears on the verge of missing opportunities offered by a new technology based on a science in which the United States is the preeminent leader.

This is a dire conclusion. The United States is a Nation built on vast endowments of both natural resources and human capital. It is not an exaggeration to say that in the industrialized world of the

1980s technological know-how provides a Nation like the United States with even more international economic power than does a vast natural resource endowment.

Yet today we are going to be told by OTA that America's ability to turn an important scientific discovery into marketable products and processes is flagging. To me, this is as serious as if OTA were here to report that our natural resources were disappearing. We are losing the ability to develop and apply technology as quickly and successfully as our international competitors. This is unacceptable if we are to maintain our technological leadership in the world and our standard of living. "Morning in America" must not be turned into twilight by apathy, inaction, or rigid ideology.

OTA has put together a series of suggestions as to what we in Congress might do about this, and we will examine these seriously and thoroughly in our discussions today. Several of OTA's recommendations center on better management of science and technology policy in this country. I am very strongly in favor of such a goal. In these times of budget deficits, we need a process to identify our R&D priorities and to ensure that high-risk, long-run, commercially-oriented projects are supported.

As many of you know, I have long advocated for this purpose an Advanced Civilian Technology Agency, ACTA we have called it, which would be modeled after DARPA, the Defense Advanced Research Projects Agency. The OTA report being released today strengthens my conviction we need to take such a step soon.

OTA also strongly recommends increased support for the education of scientists and engineers, a suggestion with which I wholeheartedly agree. The problem OTA identifies transcends our immediate difficulties responding to high-temperature superconductivity discoveries. Just as we should be concerned with building our long-run stock of industrial capital, we need to pay attention to the recent and serious depletion of this Nation's long-run stock of human capital.

OTA reports today on a crisis but it also argues that we still have a chance—a chance to reexamine and repair the research and development policies of this Nation before the race to commercialize superconductivity and other advances in technology is lost.

Our first witness today is the Honorable Dave McCurdy, Chairman of the House Science, Space and Technology Subcommittee on Transportation, Aviation, and Materials. Congressman McCurdy has been a leading student of the policy problems involved in commercializing high-temperature superconductivity and he has introduced an important bill on the subject. He has taken a lead on this in the House and we congratulate him for it and we appreciate his being here with us today.

Following Dave will be our next witness, Dr. John Gibbons, Director of OTA, who will discuss the report. He is accompanied by Dr. John Alic, Project Director for the report, and Mr. Wendell Fletcher, Deputy Project Director.

He will be followed by a panel comprised of Dr. Richard Quisenberry, Director of Research, DuPont Corporation; Dr. John Rowell, Assistant Vice President, Solid State Science and Technology, Bellcore Corporation; and Professor Malcolm Beasley from the Department of Applied Physics at Stanford University.

On our final panel, we are honored to have Dr. William Graham, Jr., Science Advisor to the President, and the Honorable Bruce Merrifield, Assistant Secretary of Commerce for Productivity, Technology and Innovation.

We appreciate all of you being here. I, unfortunately, had some things come up this morning and I will not be able to be here for the whole hearing. Senator Pryor is going to be chairing the hearing. He has expressed an interest in superconductivity and how the U.S. will attempt to commercialize it.

He will be joined by Senator Bingaman later, who has the Los Alamos labs and other important scientific research centers in his state of New Mexico.

Senator Pryor is the Chairman of our Subcommittee on Federal Services, Post Office, and Civil Service, and does a great job for us in that area.

David, we appreciate very much your willingness to fill in here this morning. Thank you, much.

Senator PRYOR. Well, Mr. Chairman, it goes without saying you have chosen a very poor substitute this morning. I will do my very best not only to preside but, more importantly, to learn from our witnesses this morning.

Chairman GLENN. Good. Thank you very much.

Senator PRYOR. I look forward to hearing our witnesses, and to joining Senator Bingaman in this effort.

Chairman GLENN. Thank you.

Before we continue I have a statement from Senator Jim Sasser who was unable to attend today's hearing but has a statement he would like inserted into the record.

[The statement referred to follows:]

OPENING STATEMENT OF SENATOR SASSER

I want to commend the chairman for conducting this hearing today to review a report by the Office of Technology Assessment concerning strategies for commercialization of high temperature superconductivity.

The issues addressed in this report are of great concern not only to the Nation as a whole, but also to my home State of Tennessee. In Oak Ridge, at the National Laboratory, we are fortunate to have one of the premier research and development facilities in the world for this type of technology—the High Temperature Materials Laboratory.

When I recently toured this facility and participated in a symposium on the role of our national labs in global competitiveness, I discussed many of these pressing issues with my constituents.

My constituents and I concur with several of the points made in this report, especially that there is a need for better communication between the universities, national labs, and private industry.

The facts are sobering. This Nation's high technology exports led the world, and indeed compensated for our trade deficit in other areas for several years. But since 1986, we began to incur a trade deficit even for high technology goods.

Therefore, now it is imperative for our country to reexamine its basic attitudes toward research and development, manufacturing, and marketing. It is time to fashion new relationships between the public and private sectors—in so far as research and development, manufacturing, and marketing are concerned, if we are to meet tomorrow's economic challenges.

We need to help our corporations and stock holders learn to look beyond the quarterly earnings report and invest more in research and development—a long-term investment. We need to consider creating more financial incentives for long-term investment in research and development, possibly through changes in the Tax Code.

Perhaps some prudent revisions to antitrust laws would enable industry to work more closely in cooperative efforts. And changes should be considered in patent laws to provide greater rewards for individual efforts.

What we see quite clearly from this Office of Technology Assessment report is that the Federal Government does not now have an effective national policy for the development and commercial application of technology.

We must do more to foster cooperative efforts between industry, universities, and the national laboratories. The Oak Ridge National Laboratory is working with the U.S. Advanced Ceramics Association to establish the Ceramic Advanced Manufacturing Development and Engineering Center to explore innovative manufacturing processes for the production of ceramic components.

As the Office of Technology Assessment report illustrates, consortia such as this are critical in our efforts to compete in the world markets.

But also, we need to facilitate the transfer of technology between the Federal Government and private industry. The lag time between discovery and licensing of that information to the private sector must be reduced.

And finally, but most fundamentally, we must augment our Nation's math, science, and engineering education programs. This includes ensuring that the National Science Foundation receives our full and unwavering support.

The price for not doing these things will be that in the coming years we will see our relative position in world markets slip further and further behind.

I look forward to hearing the views of our distinguished witnesses on these issues.

Thank you, Mr. Chairman.

Chairman GLENN. Dave.

TESTIMONY OF HON. DAVE McCURDY, A REPRESENTATIVE IN CONGRESS FROM THE STATE OF OKLAHOMA

Mr. McCURDY. Thank you, Mr. Chairman.

Mr. Chairman, I ask unanimous consent to include in the record the full text of my statement.[1]

Senator PRYOR [presiding]. It is so ordered.

Mr. McCURDY. Mr. Chairman, it is a pleasure to appear before the Committee today to discuss high-temperature superconductivity and its potential for commercialization. At the outset, I would like to say that I, too, welcome the report by OTA and believe that its conclusions certainly are not in conflict with the legislation that I have introduced. The legislation that I have introduced accelerates the effort today on high-temperature superconductors and I believe the two could work together in the future.

As you are undoubtedly aware, high-temperature superconductivity has been subject of numerous Congressional hearings, a number of which have been conducted by the House Committee on Science, Space and Technology. The most recent one was conducted last week by my colleague from Tennessee, Congresswoman Lloyd.

It is my intention today to briefly outline what we have heard in our deliberations, as well as to discuss legislative efforts that have been introduced by myself and Congressman Don Ritter of Pennsylvania. I believe much of what I say will dovetail nicely with the findings and recommendations of the OTA report you will officially receive this morning.

What we learned at those hearings confirmed much of what we had been reading in the press—that these new materials offer the potential—and I underline potential—for vast, revolutionary changes in such diverse areas as microelectronics, transportation, communications, electrical energy, and others.

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[1] See p. 41.

What was not emphasized in the press was that much work still remains. Years, possibly decades, of effort will be necessary to use these materials at their full potential. This long-term research effort will require a major long-term commitment of funds and human resources. The cost has been estimated at hundreds of millions of dollars annually in both the private and public sector.

Our analysis of Federal spending suggests that overall R&D funding for high-temperature superconductivity appears more than adequate. This research approached $90 million in fiscal year 1988 and will expand to more than $130 million in fiscal year 1989. Underlying these impressive numbers, however, we see other problems.

For instance, no national program exists. Indeed it is hard for us to identify even individual agency program plans, with DOD being a possible exception. Over two-thirds of the money is being directed through DOD. Only minimal amounts of the research is directed to processing and engineering sciences, and we believe—and I certainly support the idea—that this is essential to commercial development of these materials, and I believe it is the greatest weakness.

Industry has had little opportunity to provide formal input into current and future research plans. We have no formal long-term commitment to high-temperature superconductivity. Despite the enthusiasm shown by this Administration, there are no guarantees that the next Administration will be equally willing to pursue this work.

Let me expand briefly on one of these concerns—the lack of a comprehensive national superconductivity program. In lieu of a clear definition of priorities and goals at a national, Federal level, we have a multitude of priorities and goals, some articulated, but most not. These are directed, not surprisingly, to the individual missions as interpreted by individual agencies. Thus, they all seem to be heading in different independent directions.

Some argue that diversity is necessary and a national problem would inhibit flexibility. But just as obviously, too much diversity leads to chaos. Perhaps, more importantly, a national program would send a clear signal to our industries, such that they could most effectively develop their own programs and correlate them appropriately.

The problems caused by the lack of a national program with clearly defined priorities, goals, and commitment can be, perhaps, best illustrated by recent actions in DOD. Deputy Secretary of Defense Taft, in citing overruns in Defense outlays, this past month imposed a spending freeze on all "nonessential" DOD activities. This included window washing and lawn mowing as well as all new research and development.

Unfortunately, DARPA, although having awarded $20 million to 35 contractors as part of a Presidential initiative in high-temperature superconductivity, was able to finalize only two of these contracts before the imposition of the freeze. Extensions of the freeze beyond July 1 will prove disastrous for most of these small entrepreneurial companies. Several, in fact, faced with extinction may find it extremely difficult not to succumb to foreign pressure to sell their technology abroad.

As a point in fact, Mr. Chairman, last week we had a hearing in Armed Services Committee on the Taft memo and they are now saying there is going to be a "thaw," and especially in the R&D areas. Now, we have not seen the clear definition of that thaw yet, but this is a classic example of the lack of priority in the Administration.

A national program and commitment cannot guarantee against a reoccurrence of such a situation. However, I believe the existence of such a program, together with a clearly enunciated Presidential and Congressional commitment, would certainly minimize such problems within individual agencies.

Mr. Chairman, let me briefly mention legislation that Congressman Ritter and I, together with a number of other members, believe will address a number of concerns I have outlined here today. A measure, developed by Mr. Ritter and myself, tentatively entitled the "National Superconductivity, Competitiveness and National Security Act of 1988" has the following major thrusts: One, it calls for the establishment of a national Federal program for advanced superconductivity R&D to be developed through the Executive Office of the President.

Second, it would strongly involve the private sector in developing the national program, primarily through a National Commission on Superconductivity, created by the Trade Bill.

Three, long-term commitment to the program would be provided through the authorization of base funding of $120 million per year for a period of 5 years.

Four, research responsibilities are broadly outlined for DOD, DOE, NSF and NBS, with particular focus on the role of DARPA in processing these materials.

In closing, Mr. Chairman, let me note that even with the establishment of a national program as I have outlined here, it is not obvious that our Nation will succeed in competitively developing and applying superconductivity. However, I believe such a program is essential if we are to have a chance of success. I look forward to working with you and other interested members in helping establish such a program.

Mr. Chairman, in a last anecdote, I would comment that as I left the house this morning I was discussing with my 10-year old son—who fortunately takes after his mother, the doctor—who is involved in a program in school called "Gifted and Talented Students" in a very good school system in Northern Virginia, and I asked him what he knew about superconducting materials. Expecting probably a fairly informed response, I was dismayed that even in classes where they are taking the brightest students and giving them accelerated education, the knowledge of superconductivity is not there. And unfortunately, we compare that with the recent reports from Japan, that on the streets of Tokyo you can pick up comic books that have as the base of the story superconducting materials.

So I think we are, again, winning the Nobel prizes but we are going to lose the market share if we do not move and move quickly. I certainly look forward to working with you and Chairman Glenn and others in trying to address the problem.

Senator PRYOR. Thank you very much, Congressman.

I am afraid I am in the category with your 10-year old son, but I am trying to do some catching up very quickly.

I must apologize, but we are in a vote right now on the Senate floor on final passage of the Work Incentive bill. I am going to leave the hearing, race over, vote, and come back. So all I can suggest is everyone just relax and enjoy each other for a few moments, and perhaps Senator Bingaman will also be joining us.

Dave, thank you very much.

Mr. McCurdy. Thank you.

[Brief recess.]

Senator Pryor. Our next witness, ladies and gentlemen, is Dr. John Gibbons, Director, Office of Technology Assessment.

We welcome you this morning, Dr. Gibbons, and we look forward to your statement.

TESTIMONY OF JOHN H. GIBBONS, PH.D., DIRECTOR, OFFICE OF TECHNOLOGY ASSESSMENT, ACCOMPANIED BY JOHN ALIC, PH.D., PROJECT DIRECTOR, AND W. WENDELL FLETCHER, DEPUTY PROJECT DIRECTOR

Dr. Gibbons. Thank you, Senator Pryor.

I would submit the testimony for the record,[1] Senator Pryor, and rather than go through it in its entirety, simply summarize its contents for you.

Mr. Chairman, we are very pleased at the choice of today for the release of this report. The GPO ink is still wet on the document, so it is a fresh input to the system. We are in the middle of a companion study which focuses on research, development and application of high-temperature superconductivity, but that will not be completed for about another 6 months.

I think everyone understands, Mr. Chairman, that we are in a rapidly changing scene of events. In the first 10 weeks of 1988, two new superconducting compounds were announced. So things are changing rapidly, but at the same time it is pretty clear that the winners in this race for commercialization are going to be those who commit to a long term and sustained effort, and by sustained, we mean a decade or more.

Despite breathtaking progress, there are big uncertainties about the commercial potential for this new class of superconductors. A lot of public and private risk-sharing is going to be needed over the coming years, as well as sustained and broad-based research.

When asked once what he thought of the French revolution and the outcome of the French revolution, Mao T'se Tung thought for a moment and he said, "Well, it is still too early to tell." Clearly, I do not think it will take 200 years for the results of high-temperature superconductivity's results to be evaluated, but it certainly is also risky to be confident at this point in time.

The current situation is a little bit like a horse race. There are a lot of participants in the field from all over the world; I would say from Beijing to Buenos Aires to Moscow and everyone in between. But the key contest is between the U.S. and Japan, at least in our opinion. And the U.S. does seem to be getting off to a slower start.

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[1] See p. 48.

The two countries are going about the situation differently. In the U.S. Government we are spending in 1988 about $95 million, and most of this money is flowing through the mission agencies; about $45 million in the Department of Defense, $25 million in the Department of Energy, and about $15 million in the National Science Foundation. The total Government dollars, in other words, have accelerated rapidly over the past couple of years. There has been a good response there. And it is also larger, as far as we can tell, from the current spending rate by the Japanese Government.

The Japanese Government, on the other hand, has been supporting research with a strong emphasis on international cooperation plus direct support for commercial development. High-temperature superconductivity research in Japan is broadly based and is of world quality.

Now, United States activities in industry, of course, are quite varied. The large international firms tend to be more involved for the long pull on these things, but generally, U.S. industry is reflecting a relatively short-term focus in terms of profitability and simply waiting to see how the research and development comes out.

In Japanese industry, there is more of a long-term commitment to growth and to market share, more people are working on process and new product ideas, and they are working on superconductivity across a wide range of industries, including not only, for example, electronics and electricity, but also glass and steel. In short, Japan sees this, we think, as an opportunity to match the United States in research—and it has in the past been behind us in that regard—and also to beat us in the marketplace.

OTA came up with a number of findings concerning what we in the United States might do to improve this situation and I would like to summarize a few of those that are relevant to today's hearings. First of all, more money for commercial, non-defense R&D in superconductivity would raise the odds of competitive success by U.S.-based firms.

Second, more effective and sustained emphasis and leadership for technology transfer from the Federal labs to private industry will be needed if U.S. industry is to take full advantage of Federal R&D spending. That technology transfer has to come not only at the end of research projects, but also should involve interactions early on, even in the research planning phase.

Our third observation is that consistency and continuity in Federal support for high-temperature superconductivity is going to be needed, rather than the year-to-year sort of policies that have been more common in the past. At a time when new research results were coming in at a rapid pace—at some points as much as once a week—there were confusion and delays in R&D funding, and that made it difficult for the U.S. to keep up, and we are still struggling in that regard.

The fourth finding is that we feel that Federal policies that steer more resources to engineering and technology without at the same time sacrificing the potent advantages to the U.S. economy from leadership in basic and applied science could help American industry compete not only in high-temperature superconductivity but in many other fields. Indeed, the high-temperature superconductivity issue is but a microcosm, a good example, of the broader issues we face in moving the results of science to commercial application.

The fifth finding, Mr. Chairman, is that gaps and holes sometimes open up in the Nation's technology base where no Federal mission agency has an immediate need; for example, manufacturing processes. A proactive policy that would channel support to joint industrial R&D, proposed and carried out by private sector consortia, could help avoid such situations.

And a final observation is that Japan has offered to open their Government-supported research projects in high-temperature superconductivity to foreign participation. This is not just altruism on the part of the Japanese, but the U.S. has just as much to gain as the Japanese with respect to this kind of cooperation and participation. For us, only by participating side-by-side with Japanese scientists and engineers will we come to understand how they compete so effectively through technological innovation.

Based on such findings as these OTA developed 20 specific options on how Congress might improve the U.S. situation. Those are summarized on page 84 of the report.

Now, how we might improve the situation, not only with respect to high-temperature superconductivity but also more broadly to the issue of commercialization and competitiveness? The options have to do with such things as R&D support, strengthening interactions among and between universities, industry and Government, and technology interchange with Japan.

Then, finally, to gain a broader perspective, we outline in the report three strategic views of commercialization of new high-temperature superconductivity technology. These reflect different views of the strengths and weaknesses of the U.S. approach to technology development.

The first of these three we might call a flexible response, which is essentially a continuation of our traditional approach in which R&D is channeled through mission agencies, especially Defense. But what has worked very well in the past in that regard will not work in the future. In fact, it has not been working very well over the past 10 or 15 years.

Mission oriented R&D coupled with basic research does not provide enough support for commercialization to ensure that our firms, U.S. firms, will be able to match foreign competition. The 20 options mostly have to do with making this strategy of flexible response work better.

The second strategy would be a more aggressive approach. Greater support would be given to commercial, non-defense R&D in superconductivity, including, first, a larger role for the NSF; traditional funding of university research, but then adding support of multidisciplinary university research centers.

Second, there would be Federal cost-sharing of long-term and high-risk high-temperature superconductivity R&D, planned and conducted by industry consortia somewhat similar, for example, to the DARPA process in DOD.

Third, a working group might be established on commercialization, the working group including Government, university and industry people to help shape the R&D agenda and guide Federal financing of private-public sector consortia. The working group would need significant authority and should be set up, importantly, we feel, to last a decade. It is that kind of time that we have to commit to if we intend to win this race.

We feel that this more aggressive strategy could substantially improve prospects for rapid commercialization of high-temperature superconductivity at a relatively modest public cost—for example, perhaps $20 to $30 million a year.

The third perspective or strategy would be a more extensive Federal technology policy. This implies major organizational steps with alternatives ranging from a small independent agency to a cabinet-level department, all of these ideas intended to support the development of commercial technologies. A thorny problem with this third strategy is that absent a clear cut mission to discipline the priorities and funding decisions, such as exists in DARPA, a Federal agency could end up subsidizing marginal projects. In other words, fuzzy goals might lead to fuzzy decisions.

In any event, this third strategy cannot help on high-temperature superconductivity in the critical few years immediately ahead of us simply because of the time required to make the changes work. But even if we were too late with respect to some of the work in high-temperature superconductivity, major public policy changes could ultimately put us in a better position when the next opportunity comes along, and we know it will. New scientific discoveries such as these discoveries in superconductivity occur every decade or so.

In conclusion, Mr. Chairman, the U.S. retains many sources of advantage in international competition. We have unmatched capabilities in science and an economy that is the largest, most diverse and most robust that the world has ever known. But the traditional U.S. approach to technology development—that is, heavy Federal R&D spending, mostly by the mission agencies, coupled with indirect encouragement for innovation and technology development—no longer seems adequate. If it is not too late to craft a policy that will help American industry in the coming competition in high-temperature superconductivity commercialization—and I do not believe it is too late—by this time next year it could be. In other words, things are moving rapidly and we simply are not going to win by waiting for the next train.

Thank you, Mr. Chairman.

Senator PRYOR. Dr. Gibbons, thank you.

The Committee welcomes Senator Jeff Bingaman who will share in chairing this hearing.

Dr. Gibbons, two or three questions. One, you state—or the report states, I should say—that the dollars spent by the Department of Defense or by the Department of Energy will probably buy substantially less in terms of the Nation's technology base than a dollar spent by the National Science Foundation.

Now, how did you reach this conclusion, and why is this a fact?

Dr. GIBBONS. May I ask Dr. Alic to respond to that?

Senator PRYOR. Sure.

Dr. ALIC. Mr. Chairman, that is really a judgment based on analogies with past developments in high technology in the last few decades in which many of the foundations of the Nation's technology base can be traced to university research groups which developed, for example, fundamental understanding of large-scale integrated circuits.

Now, industry uses the technology that is generated through that Federally funded R&D, but much more of those underlying technical tools that everybody draws on and uses come from the universities than from the national laboratories or defense contractors.

Again, let me emphasize, as one moves downstream the actual development, of course, is done in a different part of the R&D system.

Senator PRYOR. On page 4 of Dr. Gibbons statement, the first sentence, American corporations have been backing away from long-term, high-risk R&D since the 1970s; Why is this?

Let me ask the question this way. One, why is this and what are the Japanese doing to grant the incentive for their private industries to go forward in this field?

Dr. GIBBONS. I would like to start a response to that and then invite my colleagues to join if they might.

First of all, the nature of financial markets are such that the managers of American corporations have to spend more and more time thinking about the next quarter and the progression of quarterly earnings than about longer-term product development. The extent of merging of corporations, and especially of buy-outs by adversarial parties also tend generally to cause the CEOs to be less concerned about some of the long-term issues and to look a little bit more closely at the R&D dollars that they might use in some other way.

So these are at least two things that have happened in industry that I believe would have to be included in any list of why the U.S. corporations seem to be backing off of the stand.

Might I ask John to comment?

Dr. ALIC. I think there is, perhaps, simply one point to add to that, Mr. Chairman. That is that in many American corporations technology and the use of technology is not inherent in corporate strategy to the extent that it is in Japanese corporations.

Now, there are many exceptions to that. Many American corporations are very aware of the potential of new technology for their businesses. But as a generalization, I think that awareness of how technology affects one's future business, and far in the future—perhaps 10 or 15 years ahead—is stronger in Japan and also, perhaps, in countries like West Germany, than it is in the United States.

Senator PRYOR. Thank you, Doctor.

Senator Bingaman?

Senator BINGAMAN. Let me ask a question about the working group which you referred to in your statement. How do you perceive that as happening? Let me be more specific. Last year we added $21 million in appropriations in the defense bill, which went to DARPA for additional work in superconductivity. We had a provision in there that they were to coordinate their efforts with DOE, NSF, NBS and NASA.

I am not disagreeing with you that we need a better coordination of the activities going on in a lot of agencies, but I just guess I would like to know how it is going to work. Someone has got to be in charge or nothing happens. I mean, you cannot fly across the Atlantic by committee.

So how do we structure it?

Dr. GIBBONS. It is easier to conceptualize the general framework that it is to make it work. You are absolutely correct. I think Mr. Fletcher may have thought about this more than I and I would like to ask him to respond.

Mr. FLETCHER. In terms of the working group, Senator, this is seen as something that would be an organization or a group that would have representatives from Government, industry and universities to have a kind of, I guess you would say, oversight role or facilitating role with respect to handling resources in the more aggressive strategy.

It would not necessarily be an entity that would assume day-to-day responsibility for coordination of programs amongst agencies. So in that sense I think it would be a little bit different than the kind of coordination effort that individual agencies might attempt to make amongst themselves.

Senator BINGAMAN. Well, I am still pretty vague about what we are talking about here. Are you suggesting that we should not have any more concentration of authority to make decisions on allocating resources than we presently have? Should we just sort of leave it spread out where everybody gets a little money, and all we should do is set up a mechanism so that they talk to each other?

Is that adequate to the job or should there be some central focus in the Federal Government where one person is in some sense responsible for the national effort in superconductivity?

Mr. FLETCHER. I think that it is useful to think of coordination in two different levels of coordination. One is a kind of agency interaction where program managers in the agencies interact on a fairly frequent basis on a kind of technical level, consulting with each other regularly and bringing up issues of a technical nature amongst them. That seems to be happening fairly consistently with respect to high-temperature superconductivity.

The more difficult question is the policy-level coordination and I think that is what your question is getting at.

Senator PRYOR. Mr. Fletcher, let me, if I may, first apologize for interrupting you. We are in a vote on the Senate floor and we have about 6 minutes remaining. I guess we will race over there and come right back.

We do apologize for this sporadic attempt to hold this hearing today. We know there is a lot of talent in this room going to waste.

We will be right back in a few moments.

[Brief recess.]

Senator BINGAMAN [presiding]. Let's get started again. Again, we apologize for the numerous interruptions.

Let me ask one additional question, if I could, and then we will go on to the next panel.

My question is on the issue of whether we should have a more coordinated directed effort in this area, particularly on commercialization. Your third strategy is a Federal Technology Agency. That is as an alternative to this working group idea that is part of your second strategy.

As you point out, a Technology Agency could not be done quickly enough to really have an impact on the development of superconductivity over the next few. It is more of a long-term need.

What can we do in the next few years to actually grab a hold of this thing and deal with it? Is there anything a little more dramatic than just having a working group? That is my concern.

Dr. GIBBONS. The main issue is the role of the public sector, that is, the Federal Government, in commercialization, which carries us beyond our traditional roles. Now, you could start, for example, with the advisory committee process for the operation of agency laboratories and involve in the advisory process strong representation of industry and university as well as Government.

Senator BINGAMAN. Advice to whom, now?

Dr. GIBBONS. To the directors of the Federal laboratories.

Senator BINGAMAN. Okay.

Dr. GIBBONS. And that could have some important results. It has already in some of the DOE labs. At the same time, the agencies' missions are always going to be dominant, and rightfully so, in the operation of these labs. So that pushes you, in the longer term—although many things could happen in the near term—in the longer term it pushes you upward toward agency advice rather than laboratory advice and ultimately toward interagency advice with clout, such as the working groups that we talk about, that would specifically look at commercialization, not only in terms of the research agenda and the funds flowing within those agencies, but also linking that process to people in the private sector who would take it and run with it in terms of commercializing.

Now, a final step in the progression of ideas would be to establish some kind of agency what would be given a responsibility to put it all together rather than having it distributed around a number of agencies.

We think there are pros and cons to that argument, but it is the ultimate in terms of this integration of effort to highlight the new felt need for the Government to reach deeper into the commercialization process by getting in closer partnership with industry in risk sharing and in generic research.

I might ask Dr. Alic if he could add to that.

Dr. ALIC. May I go back to the working group for just a moment?

Senator BINGAMAN. Sure. Go right ahead.

Dr. ALIC. Perhaps it would help to clarify our analysis to restate part of the problem that we see in high-temperature superconductivity, and that is that commercial industry, companies that are aiming at the civilian marketplace, are getting relatively little of the Federal R&D dollars—totaling $95 million in the current fiscal year.

We see that as part of the problem—lack of resources for companies, many of them small and entrepreneurial who are aiming at the civilian marketplace. The purpose of the working group, which is a supplement to existing activities within the Federal Government—it is not a replacement, it is in parallel with things that are going on in the agency right now—is really to do two things; first, to help put together a consensus on what kind of R&D investments are needed to support developments on the civilian side of the economy where the international competition will concentrate, and secondly, to provide guidance for decisions on the specific mechanisms that we discuss in the report for supporting that kind of

R&D; that is, Federal cost-sharing with industry consortia in which the ideas to be explored come from industry.

The working group mechanism stands alongside and supplements the things that we are already doing. Their function is new and, again, we see that as an experiment that then might have application to other technologies.

Senator **Bingaman**. And who do you see as the right official to establish and convene such a working group?

If it is everyone's job, it is nobody's job.

Dr. **Alic**. Well, to put it simply, there is no right place for it because it is a function that we have not recognized and acknowledged in the past. It is something new. Therefore, in our report we go through the alternatives and conclude that for administrative convenience, the best choice is probably to attach this to the Office of Science and Technology Policy.

But we see the locus of decision-making authority in the working group itself and not in some existing part of the Administration.

Senator **Bingaman**. Senator Roth?

Senator **Roth**. Dr. Gibbons, I regret I was unable to be here earlier. Perhaps a few of my questions have already been touched upon, but I wonder if you could comment on where you see this country vis-a-vis Japan and vis-a-vis Europe? Certainly the traditional view is that Japan perhaps in certain aspects is leading us and that Europe is falling behind.

If that is true, are we better off, in the case of Japan, trying to work with them in some cooperative manner or are we better off trying to go it alone? And from the standpoint of Europe, why are they falling behind, if that is the case, and what should we be learning from that? What should we be avoiding, in other words? And I would be interested, Dr. Alic and Mr. Fletcher, in your comments.

Dr. **Gibbons**. Thank you, Senator Roth. I mentioned in my testimony that there are a lot of horses on the field but the real race in this particular regard is the U.S. and Japan. Europe, Japan and the U.S. are all doing world-class research in high-temperature superconductivity. Europe suffers from a seeming lack of ability, somewhat reflected in the U.S., of moving some of these ideas into the competitive commercial marketplace.

One can see this in the results——

Senator **Roth**. Could I interrupt there?

Dr. **Gibbons**. Yes, sir.

Senator **Roth**. Are you saying, European basic research is essentially comparable?

Dr. **Gibbons**. Their basic research is excellent.

Senator **Roth**. In this area?

Dr. **Gibbons**. Yes, sir. Japan has traditionally been seen to be very strong in commercialization but, on the other hand, perhaps less innovative and strong in basic research. In this regard, Japan is seeking to elevate their capabilities to be on a par with the rest of the world research community. And so we see Japan catching up in research in which the United States and Europe have largely been dominant for many years.

We see Japan being interested in cooperation in basic research and in some generic research areas. They do this for several reasons. For one, it is being urged on them by the western countries to participate more fully in the world advance in basic knowledge. They also see it as a very important mechanism to infuse innovation, new ideas, new thinking, into the Japanese research community. So they do have a self-interest in this kind of openness.

At the same time, as I pointed out in my testimony, the United States has a self-interest also in participating for it could give us insight into the way science and innovation occurs in the Japanese culture, which is very important for us to understand better.

Senator ROTH. Let me take that one step further. How about its utilization? Again, the traditional view is that the Japanese are much better at commercialization. Do you think we should proceed from research and development and try to work out some method of cooperating in the utilization?

Dr. GIBBONS. I believe the Academy of Sciences is presently looking at this question of comparability for sharing. For example, much excellent work is done within Japanese industry, but it is outside the research community where information is generally shared. And so if we want to have a real quid pro quo with the Japanese, it cannot just be our universities sharing with their universities, where our universities do much more than Japanese universities.

But if one could develop a quid pro quo with the Japanese for comparable sharing of ideas, I think it could be very helpful for the United States in the long pull.

I might ask my colleagues if they disagree with me. They usually do. [Laughter.]

Senator ROTH. One more question. The Japanese, of course, have been very successful in acquiring the technology, the research from abroad, rather than developing it at home. There has been some concern here that because of the openness of our society what we do at the universities, or even the Government laboratories becomes available to our competition as well as to our own business interests.

Do you share that concern? And in the alternative, now that Japan is obviously moving to become a major factor in research and development, will we have the same kinds of access to their research, their technology? If not, what can we do about that?

Dr. GIBBONS. Senator Roth, that is a very important point, namely the differential access of scientists and engineers in the two countries. That is reflected in the fact that there are nearly 15 times as many Japanese in the United States today in our universities and industry than there are Americans in laboratories in Japan. I think the number is something like 7,000 Japanese over here and 500 of our people over there.

One of our policy options speaks to the attractiveness of thinking about how we can train more of our budding scientists and engineers also to be fluent in Japanese. Because it is one thing to visit a Japanese factory or university and speak in English. It is quite a different thing if you can go over and speak in their language in terms of real access to information.

So we are suffering from, in a sense, a disadvantage in that our language is a world language and Japanese is not.

Senator ROTH. That is all the questions I have. Thank you, Dr. Gibbons.

Senator BINGAMAN. Senator Chiles, did you have questions of this panel?

Senator CHILES. Mr. Chairman, I just want to congratulate you on holding the hearing. The main thing I am trying to determine is the panel's feelings as to what we need to do and I think part of that is in the statement, so I will reserve my questions.

Senator BINGAMAN. Why don't we go ahead and allow you folks to be dismissed and call up the second panel. Thank you very much for the report.

In our next panel we have Dr. Richard Quisenberry, Dr. John Rowell, and Professor M. R. Beasley.

Senator ROTH. Mr. Chairman, may I just welcome Dr. Quisenberry, who is the distinguished scientist and leader of research at DuPont Company, which I think history will show was one of the first major companies to spend a large amount for research and development and that it has been key to their success down through the years.

It is indeed a pleasure to have you here today, Doctor.

Senator BINGAMAN. Why don't we go in the order in which I introduced you. Dr. Quisenberry, do you want to go ahead, please.

TESTIMONY OF RICHARD K. QUISENBERRY, PH.D., DIRECTOR OF RESEARCH, DuPONT CORPORATION, WILMINGTON, DE [1]

Dr. QUISENBERRY. Thank you, Senator Roth.

Mr. Chairman, I am Richard Quisenberry, Director of Research for DuPont's Central Research and Development Department in Wilmington, Delaware.

I thought what I would do this morning in that we have heard and read in this report some things that industry is doing and should be doing. I thought I might, for the Committee's benefit, tell you what at least one company is doing, as an example, and why we are doing it, and then we can go from there.

At DuPont responsibility for management of our very large R&D effort is distributed over a wide range of business and technology interests. The work we are talking about this morning on high-temperature superconductors happens to be in the corporate laboratory for which I am responsible. This corporate laboratory is the largest and, we hope, one of the most productive basic research units in the chemical industry.

Our mission is to support the long-range—and the emphasis is just that—the long-range technology interests of DuPont and to develop new applications—including initial commercialization—for new technology.

In this organization we have a tradition of basic research, applications development and successful commercialization of new materials of many kinds—fibers, films, plastics, ceramics, imaging technology and materials for electronics, and most recently, biotechnology.

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[1] See p. 66 for Dr. Quisenberry's prepared statement.

High-temperature superconductors appear to us to fit the mission of this corporate laboratory and accordingly we have mounted an effort which we believe to be the most substantial among the world's chemical companies.

This morning I would like to take a few minutes and outline for you the scope of our program. I will indicate how we have organized it and why; and, along the way, I would like to offer you a few observations on the report just issued this morning by the OTA.

The research and development programs in high-temperature superconductors at DuPont has three components; one, basic studies on the nature of the materials; two, the demonstration and early market testing of potential applications; and three, product planning and market evaluation.

The OTA report also characterizes these three elements as the hallmarks of the Japanese approach to new materials. I would point out that these have always been characteristic of successful production introduction from the results of basic science in our company and in others. I know of no other way to do it. The notion of monitoring a fast-moving field and then proposing to jump in at the right time just does not work. Such a strategy might be successful in a mature technology but certainly not in this field of high-temperature superconductors.

In our basic research organization, we have broad scientific expertise and facilities in chemistry, ceramics, product characterization and solid state physics. These technology elements have been put in place over many years to provide materials to the electronics industry, pigments to the coatings, paper and textile industries around the world and for discovery of new catalysts for more effective synthesis of chemicals.

Within this effort had been a group working on superconducting materials. Within days of the first successful reports of high-temperature performance that came out in the scientific journals in January of 1987, we were able to redirect things and mobilize an interdisciplinary task force to understand and extend the results that were then being reported from Zurich, Tokyo, and Houston. We have been among the first to identify accurately the unique physical structures which account for superconductivity in all the various chemical classes thus far discovered. We have developed attractive chemical syntheses for these important materials. We have in operation a semiworks facility to prepare high-purity material for our own research and for that of our collaborators in other laboratories.

As the OTA report correctly points out, the important commercialization step of high-temperature superconductors will not be in the materials themselves but of their applications. Here again, many of the applications which have been visualized for these materials exploit manufacturing and processing capabilities we now have in place at DuPont for many other products.

To mention a few: Shaped articles in the form of wires. We believe we can draw substantially on our years of development of ceramic fibers. Similarly, our experience with ceramic-based composites is proving quite useful as we seek new applications. Synthesis and characterization of the hard-to-manage precursors of the superconducting materials themselves is another strength we think we bring to this technology. Finally, we have broad background in laying down layers of materials for electronic applications, so-called thin-film and thick-film technology.

For all of these above reasons, we feel quite comfortable in justifying a long-range science program. In this effort, we have established collaborations with scientists at important universities, such as Houston, MIT, Stanford, California-Berkeley, California-Santa Barbara, CalTech and the University of Virginia.

With regard to the second element—product and applications development—experience with other new and evolving materials teaches us an important principle, and that is that despite the most thorough product planning and analysis, the marketplace contains many surprises. So the sooner we test the marketplace the faster we learn and can move ahead. Thus, it is critical to test out and try out conceptual applications with potential users and customers as early as possible.

We know that some of these early trials will fail commercially, and others will succeed. Again, as the OTA report states, this is a well-used concept in Japanese high-tech industry. To this end, we have established at our experimental station in Wilmington an applications concept facility for high-temperature superconductors. It is staffed by scientists and engineers with extensive and varied backgrounds in materials, processing technology and electronics applications. Its purpose is to enable any scientist at DuPont with a reasonably good idea for superconductivity applications to come forward and test his concept with the aid and guidance of experts. Such an approach is novel in our company and its objective is to speed technology transfer from the concept to the demonstration stage.

I think—to return to the OTA report for a moment—I would observe that the overriding criticism in this report, that seems to me, is that technology transfer to the commercial sector from our basic laboratories, be they in industry, academia or Government, has been slow and unsatisfactory. If this is the conclusion of the report, I would concur with it. Better ways to effect technology transfer between ourselves and outside laboratories is a major item on the agenda for we who manage research at DuPont. There are formidable barriers to such technology transfer and they go well beyond the well-publicized "not relevant" or "not invented here" excuses.

Academic and Government laboratories have traditionally not had a technology transfer mission. Scientists in these laboratories often have an understandably unrealistic view as to what it takes to commercialize something. We find that if we wait for proposals to be advanced by these laboratories, they are often late or lacking in scope. We conclude we must put mechanisms in place to extract technology from these laboratories in a more timely way. In doing so, we encounter a maze of university policies, indifference, and in some cases, restrictions associated with Government funding already there. None of these is impossible to overcome, but they do slow the process.

The final element of our program—product planning and business development—involves a third group of scientists who have substantial business experience. They do three things for us: They define and quantify business opportunities, they identify potential development partners, and they help us set our priorities.

It is clear to us that if we are to be commercially successful with high-temperature superconductors, we must develop research partnerships with others and ultimately commercial relationships. We are well on our way to forming many such relationships. Some are with large electronics and computer companies, others are with companies with experience in imaging, and still others are with small so-called start-up firms with scientifically intriguing, but high-risk, applications.

We also belong to consortia. We have joined the MCC superconductivity consortium in Austin, Texas. Through our Japanese subsidiary, we belong to the MITI consortium described in the OTA report. We belong to CSAC here in Washington, and we are finalizing plans to affiliate with several university-centered consortia.

Our program this year is budgeted for about $10 million. All of this is underwritten as long-term corporate research and is not assessed as a cost on the bottom line of any business manager in our company. I note that the OTA report holds that U.S. industrial reluctance to staff up for high-temperature superconductivity derives in part from unwillingness of business managers to sacrifice short-term performance for long-term opportunity.

Our company would not be where it is today if earlier managers had adopted such an expedient. Most of our products are materials which others use—increasingly in high tech applications. In almost every case, finding and then qualifying these for commercial approaches has proven to be an expensive process involving years of work. The innovative culture remains with us. There is no question in my mind but that achieving a world-scale materials business, including being the low-cost producer—which, incidentally, is the true measure of competitiveness—requires this long-range vision. Without such vision, American industry may lose this race and others to come.

Thank you.

Senator BINGAMAN. Thank you very much, Dr. Quisenberry.

Dr. Rowell?

TESTIMONY OF JOHN M. ROWELL, PH.D., ASSISTANT VICE PRESIDENT, SOLID STATE SCIENCE AND TECHNOLOGY, BELLCORE CORP., RED BANK, NJ

Dr. ROWELL. Mr. Chairman, thank you for allowing me this opportunity to comment on this Office of Technology Assessment report on commercializing high-temperature superconductivity. As I have spent the whole of my technical career of 27 years working in or involved with, this field, it is, of course, exciting for me to see the field receiving this level of scrutiny and attention. Due to the discoveries that began in 1986 and continue today, my old area of research and technology has indeed changed, both scientifically and politically.

Let me say at the outset that in general I agree with the findings and conclusions of this OTA report. It is the result of careful research into the current situation in this country and Japan. However, I believe it is somewhat more than that. It is not just budget numbers and head counts. It reflects a little of the underlying excitement and hopes and fears and frustrations of those in the field today. It has taken the pulse of the field and found it to be generally healthy at the research frontiers in both countries, but has also identified some areas of concern for the long-term health of the field in the United States.

Thus my intention here is to add a few personal impressions of the health of the field, and perhaps disagree with a few points of emphasis, largely from the perspective of industry, although I should say that my career in industry has been much closer to research than to commercialization.

I will therefore address four questions—funding, personnel reallocation to superconductivity from other fields, the industrial research scene as I see it in the U.S., not in Japan, and the proposals to strengthen industrial research in this field.

First, the report emphasizes the value of NSF funding in solid state science, and superconductivity in particular. While the work of my own organization at Bellcore does not rely at present on funds from NSF, DOE or DOD, I fully share this concern over the adequacy of NSF funds. I do sense among the community at large that I interact with a great sense of disappointment over the funding situation that has developed in the past six months.

To understand this you should place yourselves in the shoes of the individual scientists who, I think, genuinely believe recent events in superconductivity represent the most exciting scientific breakthrough they are likely to witness in their careers. Further, most of them suspect that significant technology will result from these advances in material science, even though they are not yet sure what it will be and they wish the press would stop promising it will happen soon.

Against this backdrop comes the realization that despite all the hoopla of Federal conferences, a speech by the President and promises of specific programs, essentially no new money is flowing into superconductivity research. Worse, the kind of research that created these new materials, that carried on by one or two people or a small group, has been hit particularly hard by the problems at NSF. To compound this problem, the small sums of new money that were expected to be available from DARPA have now been caught in the DOD freeze on all new research contracts. Is it any wonder that these scientists are asking what they have to do or what field they have to be in in order to be supported?

Perhaps an analogy to bring this point home is to compare the present state of superconductivity to the early days of NASA's space flights and imagine the reaction if at that time there had been no new money to continue the space program.

Second, the report points out that the funds that are flowing into superconductivity research—and they are significant—are reallocated funds. The funds imply a movement of people from other fields to superconductivity. My concern and question is, what is happening to these other fields? Are we so confident of our technological leadership in all fields of solid state materials science that we can afford to divert $150 million worth of funds and people away from them?

Let me illustrate my point with an example from my own organization at Bellcore. Our program in superconductivity relies very heavily on the talents of one gifted solid state chemist, Jean-Marie Tarascon. He has been the heart of our program. Before late 1986, when the superconductivity field broke open, he was doing creative work in the discovery of new materials for solid state batteries. Now that he is working with superconductors, I have no work on batteries, even though they are important. Replacing the battery research is not only a budget question, it is also a personnel question, namely finding another Tarascon.

I am sure this scenario is being repeated many times over in U.S. universities and industry. We have only limited budgets and personnel. If we have 10 people working in superconductivity, we have 10 less in other fields of materials research. What fields are they? Who has judged that they are less important to national competitiveness than superconductivity?

I hear that Argonne National Laboratory has 100 people working in superconductivity. What were they doing before? Whatever it was, it is clearly not being supported now.

Again, using a local example, I have no doubt that in the telecommunications field the impact of optical fiber, III–V semiconductors and optoelectronics—that is the integration of optical and electronic functions on one chip—will have a greater business impact than superconductivity, as long as superconductivity remains an effect found only at relatively low temperatures.

Is the U.S. science and technology base in optoelectronics so healthy that we can weaken it by diverting people to superconductivity? Nobody would claim that. In fact, our competitive position in optoelectronics is much weaker than in superconductivity.

So, as we emphasize superconductivity and use it as an example of our strengths and weaknesses, let us not ignore all the other sub-fields of materials science and solid state physics from which have come so many inventions that changed our way of life and economic well-being. In fact, I think it is legitimate to question why this report implies that superconductivity is the one field of materials research that requires special initiatives for its commercialization. It is obviously receiving the most attention. How do we know it is the most important?

Third, the report carefully analyzes industrial research in superconductivity in the United States. Let me give you my impressions based on feelings, largely, and one simple analysis. In U.S. industrial research, we have the major league and the minor league. Unfortunately, the major league only has two teams—AT&T and IBM. Then we have the minor league, which does not have many teams either. There is Bellcore, which I like to think is the most productive group in the field, given its small size, but you will realize I am not objective on this point.

Then there is DuPont, which has begun to commit a significant effort, but has not yet contributed to the field, say through publications, at the level of AT&T or IBM. This minor league is complete with Exxon, Xerox, Ford and Westinghouse, so has six teams, although a number of other companies are beginning to get involved, as the report shows.

Now, I base my assignments to the major and minor leagues on an analysis I completed of the talks presented at a recent Physical Society meeting, in fact, the one held a year after the "Woodstock of physics" meeting that is referred to in the report. Now, of 620 papers presented on superconductivity in this meeting, the industrial research contributions were AT&T 38, IBM 37, Bellcore 10, DuPont and Exxon five each, Ford and Xerox four, and Westinghouse three.

For comparison, from the Federal labs, both Los Alamos and Argonne presented 17 papers, Naval Research Lab 13, Brookhaven 12 and Bureau of Standards eight. Hence, the total output from national labs was comparable to that from AT&T and IBM combined.

Now, I would remind you that AT&T and IBM are communications and computer companies with their technology based on electronics and evolving towards optoelectronics and optics, in the case of AT&T. These are also largely Bellcore's interests. My question then is, what will happen if the major application of the new superconductors is not in electronics, but some other field, say transportation, for example? Which U.S. companies will then assume the leadership role being taken today by AT&T and IBM? To put it bluntly, if AT&T and IBM were not in this field, U.S. industrial research would be in deep trouble. Although these comments are specific to superconductivity, I believe they apply to many other research areas also.

I feel that, as an industrial research manager, I should also respond to the statements in the report that we have been tentative of our support of superconductivity. I remind you, our situation is no different from that of the funding agencies. We, too, have fixed budgets and no new money for superconductivity. Our scientists have also dropped other interests to work in superconductivity. The shift of even 5 or 10 people into this field within a research organization of say 100 is a very significant and visible shift, given the many other fields that we feel it is essential to maintain activity.

For many research managers, 5 or 10 people is a limit beyond which real distortions of other valuable programs would occur. Thus the criticism of tentativeness in industry should not be aimed at its level of superconductivity research but its overall support of long-term research in all scientific fields.

Fourth, let me comment on the suggestion made in the report that Federal funding should be used to encourage industrial research, say $20- to $30 million per year "well spent" at the same time, as, "they, industry, are turning away from long-term, high-risk R&D."

It is here that I am uncomfortable with the report, much as I would like to have Federal funds to support an expansion of our programs at Bellcore. My feeling is that unless you find ways to change the mind set and vision of those who are turning away from high-risk industrial R&D, providing funds from Washington will simply result in a reduction of the research funds that the company presently provides internally. You will give them an excuse to do even less themselves. Thus creative ways must be found not only to stimulate industrial research, and especially knowledge transfer to industry from our outstanding university programs, but at the same time to ensure an increasing corporate commitment to research and development on the part of those who receive these funds. If creative funding mechanisms cannot be found, the long term impact of helping industry is likely to be negative.

I would point out that there are strengths in the U.S. industrial scene that should not be overlooked if funding does become available in the way proposed. We do have a superconducting technology industry today. We have companies making products of low Tc superconductors, but most of them are so small you had not heard of them until you read this report.

Another strength is the freedom of university professors to consult with industry, which until recently was not allowed in Japan. Professors can also create new venture companies. In fact, most—maybe all—of the ones you have heard of, that are being formed to commercialize the new superconducting materials, have indeed come from universities rather than from U.S. industry or Federal labs. These small companies have competed very effectively for the United States in the past. In fact, I find it both interesting and sobering to note that these small companies at one time represented our entire commercial effort in superconductivity, despite the fact that it was much larger companies, such as Westinghouse, RCA, AT&T and IBM that supported the research which made their products possible.

If the applications of the new materials grow slowly, as many of us expect, then I see no reason why these small companies, or new ones like them, should not grow adequately and be able to compete as the technology grows. Thus the use of any Federal funds for U.S. industry might be best directed to these small firms, as they have already demonstrated their commitment to making superconductivity work and to making money from it.

You will have noticed that I have said very little about our competitive position versus Japan, which is a major theme of this report. I do wonder about this emphasis on competition, especially as I do not believe the field is even close to commercialization. When I was in Japan a month ago, I asked the leader of their new consortium why Japan had so many groups working in superconductivity. His reply was, "Because the United States has so many strong groups," and he seemed to be serious. Perhaps we are getting carried away by an obsession with competition on both sides of the Pacific.

If there is one lesson we have learned from the work of the past year, it is that these materials are not easy to engineer into useful forms. They may be easy to make, but making them practical is much more difficult. There are no superconducting and flexible conductors with adequate performance for any application. There are no competitive electronic devices of any type. Thus the field is presently a research field. My view of this research field is that both the United States and Japan, and other countries, have made significant contributions. At this stage there is more opportunity for research collaboration than for industrial competition.

Thus we should give our immediate attention to keeping our research programs healthy, for example, by increases in NSF funding, not only in superconductivity but in materials research more broadly. However, I do share the concerns expressed in this report that U.S. industry does not have the ability to exploit this research base in the long term. I might go further and question even the desire to exploit it. Who in industry on this side of the Pacific believes that "he who controls materials controls technology?" Hopefully one result of the attention being given to superconductivity, as in this report, will be to focus our attention on materials research capabilities in the United States, in universities, Federal labs, and particularly in industry. If so, the authors of this report deserve our thanks.

Thank you.

Senator BINGAMAN. Thank you very much, Dr. Rowell.

Professor Beasley?

TESTIMONY OF PROF. M.R. BEASLEY, DEPARTMENT OF APPLIED PHYSICS, STANFORD UNIVERSITY, PALO ALTO, CA

Professor BEASLEY. Thank you, Mr. Chairman.

Before beginning my remarks, I would like to thank the Committee for giving me an opportunity to comment on this OTA report. As a university professor who has spent most of his career working in the field of superconductivity, and who is deeply involved in the current whirlwind of activity in this field, I appreciate being able to share with you some of my thoughts and perceptions, to give you the view of a worker in the field, so to speak.

Let me also say at the outset that I find myself sympathetic with the general tone and much of the analysis of this report. It is gratifying that Congress is receiving a realistic and balanced picture of the nature and prospects of high-temperature superconductivity, despite the often hyperbolic accounts in the popular press.

The report is built on two basic premises. First, that there has been a major scientific breakthrough, the long-term technological impact of which is almost certainly large but currently unknowable with any precision; and second, that the institutional arrangements and traditions established in the aftermath of World War II for the commercialization of science and technology in the U.S. are no longer working well.

These premises appear to be widely accepted. I personally believe them to be true. But as recognized in the report, the scope of the second premise is much larger than the first. Thus, at the expense of stating the obvious, some actions that may be appropriate in general for enhancing the economic competitiveness of the country may not be appropriate in the particular case of high-temperature superconductivity, given its present early state of development.

For example, large engineering demonstration projects are not appropriate at the present time for high-temperature superconductors, whereas they certainly are for low-temperature superconductors. Emphasis on materials and materials processing and on prototypical magnet conductor and electronic device development definitely are appropriate. Concern with manufacturing technology would be premature, in my judgment, important though it may be in the overall U.S. competitive position. In short, the initial emphasis should be on generic research and on establishing the technological base for applications that one can expect to emerge in 5 to 10 years.

In their report, OTA analyzes the U.S. and Japanese responses to the opportunities presented by high-temperature superconductivity. Much of their analysis relates to the response of industry in these two countries. I am happy to have left commentary on those particular aspects of the OTA report to my industrial colleagues on the panel.

I would like, however, to stress to the Committee a point not emphasized explicitly in the OTA report. Even prior to the recent breakthroughs in high-temperature superconductivity, the Japanese were very active in superconductor science and technology. Their activities included major industrial engineering programs, excellent programs in their national laboratories, and a national thrust in superconducting materials, which had the express purpose of seeking higher-temperature superconductors. It was one of several such programs aimed at fostering greater strength in Japanese science.

I do not wish to suggest that the Japanese were prescient about the recent breakthrough in high-temperature superconductivity. I do wish to stress that they have had a broad, long-standing commitment to the field and that a healthy infrastructure for R&D in superconductivity existed in Japan at the time of the discovery of the new high-temperature superconductors. It is not surprising, therefore, that they reacted swiftly and are competing so effectively.

By contrast, as noted in the OTA report, the infrastructure for R&D in superconductivity in the United States has seriously atrophied over the past decade. It seems clear, therefore, that whatever else Congress and the various Federal agencies do, it is essential that the U.S. infrastructure be rebuilt expeditiously.

In a similar vein, given the demonstrated staying power of the Japanese, it is essential that there be continuity in R&D funding in this field so that reasonable planning is possible. The need for adequate levels of support and for continuity of that support are clearly stated in the OTA report. Emphasizing that point further here by someone from a university may seem facile and self-serving, but if we researchers in the field are to compete internationally while simultaneously attempting to develop effective mechanisms of technology transfer between universities, industries and national labs, some assurance of continuing support for the field will be necessary. Multi-year funding patterns are called for.

The OTA report concludes that the university research community is one of the strengths of the U.S. research and development establishment. That is gratifying for those of us who are part of that community. We are, however, criticized for perhaps not being successful enough in multi-disciplinary research, which the OTA report correctly points out will be essential in the area of high-temperature superconductivity.

While it is true that U.S. universities are still learning how best to conduct multi-disciplinary research in a traditionally disciplinary environment, many universities do it very well and are actively exploring new and better approaches, particularly in the area of materials research, which is at the center of all high-temperature superconductor work.

I would judge we are on the whole less successful in technology transfer. The very large companies hire our students and the smaller ones sometimes involve faculty directly, either as consultants or as part of start-up companies supported by venture capital. These modes of technology transfer are already occurring in high-temperature superconductivity. They can be quite effective. The most serious problem lies in transfer of technology to intermediate sized companies. The situation in high-temperature superconductivity is not different so far, in my experience.

Frankly, I am not confident that I know best how to solve this problem. Several suggestions are outlined in the OTA report. I am sure, however, that it can only be done through direct contact of people. Technology transfer is a body-contact sport. Universities train people and generate new knowledge. They are now well-suited for generating prepackaged technologies. Whatever mechanisms we develop or whatever policies the Federal Government adopts, the principle of direct people-to-people contact should be honored.

It should be noted that this lesson is not lost on our Japanese colleagues. They are currently willing to send some of their best researchers to U.S. universities, including a full salary and direct financial contribution to the research expenses of the host institution. At the same time, some Japanese companies, where most of the research strength in Japan resides, are now willing to accept U.S. post-doctoral researchers and visiting scientists into these laboratories.

I am in agreement with the OTA report that we should pursue such opportunities aggressively. Having worked for 2 months in a Japanese industrial laboratory, I can attest that it is a remarkably instructive experience. Also, there is the obvious point that you begin to establish personal contacts that provide a continuing window on Japan, as we have enjoyed in the case of western Europe for some time.

Finally, let me blatantly make a pitch. I would hope that in light of the strengths of university research noted by the OTA report, some additional funding could be made available for university researchers. If such funding were to become available, I would suggest it be administered to achieve the following goals: To create a few university-based centers of excellence in high-temperature superconductivity; to increase the amount of funding available to individual researchers working in high-temperature superconductivity; and to permit modernization of the research facilities in the universities.

These goals are not simply political. They reflect my honest assessment of the needs of the field. Neither scientific nor technological advances in high-temperature superconductivity will be possible without good materials and materials processing. These new superconductors are complex materials. They can contain as many as five chemical elements in their basic atomic structure. It is simply a sine qua non for progress in the field of high-temperature superconductivity that the materials problems be solved.

The problems are such that a multidisciplinary approach with a critical mass of effort is required. Physicists, materials scientists, chemists and engineers need to be involved in the now familiar pattern of modern materials research. Multi-million dollar budgets will be necessary in such centers to get the job done.

However, given that good materials or procedures for making good materials are available, individual researchers can bring their various specialized talents and tools to bear. Superconductivity is basically small science and the paradigms of small science are different from those of big science. Vitality will follow from a pluralistic approach.

Also, given a flow of reliable experimental data on good materials, theoretical physicists can be expected to continue to make progress in understanding the mechanisms of high-temperature superconductivity, perhaps shedding some light on promising roots for superconductivity at even higher temperatures. Thus by no means do all researchers have to be part of a center or a large research group to contribute to the field.

The need for improved experimental facilities in the universities is well documented and should need no further justification here.

Increased support for university research groups can be expected to have an additional important benefit. Most of the major research universities are currently beginning the process of replacing the faculty that were added in the expansion days of the 1960s. Thus, a large fraction of the faculty that will be the mainstays of our universities in the early part of the next century will be hired in the next 5 to 10 years, just the period we anticipate that high-temperature superconductivity will be a major area of basic science and engineering research. The financial support structure to nurture those faculty would greatly benefit from additional funding in the broad areas of science and engineering research that bear on high-temperature superconductivity.

Finally, let me turn to the major Federal strategies outlined at the end of this report. Of these the second, so-called more aggressive, response embodies most of the elements I have tried to stress in my remarks. On balance, I believe it needs to be examined carefully. There is, however, one element in this approach that I would like to comment upon further. That is the proposal for a working group of experts to help shape consensus on high-temperature superconductivity R&D priorities.

If this group were truly distinguished and had the blessing of both the Executive and Congressional Branches of Government, it might have merit. It would certainly have merit in my mind if its existence could ensure the balanced, long-term support that will be necessary to bring to technological fruition this wonderful event in science that has so excited all of us over the past many months. Something will be necessary to give us the staying power. I fully believe it will be worth the effort.

Thank you for your attention.

Senator BINGAMAN. Thank you very much. We are in the middle of another vote so I think we will have to adjourn for just a few moments until one of the Senators who has already gone to vote returns and then we will start with questions. Thank you.

[Brief recess.]

Senator ROTH. The Committee will come to order.

It is my understanding that Professor Beasley, you have completed your testimony, so in the absence of the Chairman, I will proceed with my questions. First of all, I want to express my appreciation to each of you for being here for what I consider an extraordinarily important problem as well as an opportunity.

One of the questions I would like to ask, because it is on the minds of so many, is in regard to our competitive position with the Japanese. As I understand from your testimony, what you are saying is we are really basically still in the research aspects of the problem.

I do not know whether it has moved on to development in any manner or not, but only preliminarily, but whether it would be desirable for us as a country or solely in the private sector to try to work out some accommodation and cooperation with the Japanese, or that it is better to leave it continue determined by the individual companies and universities.

Does anybody care to comment? Yes, Dr. Rowell.

Dr. ROWELL. I think you summarized our feelings about the relative strengths pretty correctly. In the research area we would probably regard the United States as having more strengths in the university community, and Japan has some stronger, or a good number of larger industrial groups with that research strength. But in total, I do not think the major contributions to the field, if I look at everything that has been done so far, I do not think they are that different.

You can always point to good programs over there, and we probably do, because we are kind of worried about them. But I think they in turn can point to good programs over here. So I think the research situation is pretty even. I think the difference will be as we go on and, as Professor Beasley said, the base has been there in Japan for some time and they can build on it, where in industry, certainly, we will have to rebuild ours.

Now, as far as collaboration goes, you know, we have been invited to join a number of these centers and informal collaborations between companies is quite possible. At Bellcore we have a number of collaborations with Japanese companies, research collaborations. The problem is finding people on our side who are able to take advantage of them. We have many invitations and we have many credits, because they have sent people to work with us. So we would have no problem in sending more people to Japan if I had more researchers who were willing to go there and spend a year and also master the language. That is the particular problem. Right now I have one person who is about to go and I have one who wants to go when he comes back, but those are the first two people that I have found who really wished to go and work in Japan.

You can arrange collaborations at other levels between companies, I suppose, but I think the only way it really works is this low-level interaction between the scientists themselves, and that is going to be hard, given our staffs. That is my feeling.

Dr. QUISENBERRY. I think Senator—to get to your question again—I think it is a mistake to see this in terms of a win-lose race between U.S. science and technology and Japanese science and technology. Globalization is going so fast in science that it has to be shared, and no one is going to win at the exclusion of the other, I think.

I think what we have to guard against is that the—as this report that we are discussing this morning identifies—is that the commercial consequences of this technology and all of the good things and creation of wealth in a Nation that flows from commercialization of new industries, that that occurs here as well as it undoubtedly will occur in Japan and probably Europe.

I think, from a science point of view, there is no question—and I agree with my colleague here—we will cooperate with the Japanese and Europeans, but we do not want to put any institutional or other artificial barriers in front of the commercialization. So science is going to go forward with our colleagues in Japan and commercialization I think will go forward on both sides of the Pacific. I do not see it as win-lose.

Senator ROTH. If I understand your point, what you are saying is that really the Government should not be interceding, that the cooperation is coming from the private sector? Or is there more we should be doing?

Dr. QUISENBERRY. I think Government has some things it can do. I am not suggesting that Government should stand back. I think that it should recognize that science cannot be stopped at the water line. Communications around the world are too good. Everyone reads the literature. We have a laboratory in Japan. We are going to do superconductivity research over there.

I think the Government policies ought to do really three things: one, do a better job of selection where the basic research goes; two, provide incentives of all kinds, not only financial support but accounting incentives and others, that would enable business managers to put more value in funding long-range research; and three, put incentives in front of universities and industry to do a better job of collaborating on technology transfer.

And if I could observe, I think the Japanese are handing us our lunch only in the area of technology transfer. All the pieces are in place here and we should be able to do a far better job, and we can and I think there are some things Government can do to accelerate that.

Professor BEASLEY. I agree completely with almost all that has been said. As for collaboration and joint research with the Japanese, in my own experience, it is simply that we are just not yet very sophisticated about it. There is more opportunity there than we have learned to take advantage of.

I think there is one important point you have to bear in mind, though, as you get a little bit more sophisticated about interacting with the Japanese. In Japan, much of the research strength, more so than in the United States, say, resides in the industrial laboratories. So I think that just university-to-university exchange would not be comparable. I mean, as much as such exchange would be desirable and given the emphasis that they are now putting on their academic science, basically, we need, both from academia and from industry to get more collaboration, more contact with their industrial research labs that are beginning to open up. I think that there is more opportunity there, as Dr. Rowell has said, than we have been able to or have found the ways to take advantage of.

I also agree very much that the key problem from our end is the technology transfer, and I think that many universities—certainly my own—are aware of this problem and are trying to find ways to deal with that.

The problem is not, I think, recognizing the issue or having the will to do something about it. It is more a question of finding a way to do it that is commensurate with the nature and the long-term goals and values of the university. It can be done and we must work at it. It will be done.

Senator ROTH. If I might go back to the problem of personnel and the lack of those that speak Japanese, is this a critical enough problem that it needs to be addressed from the Government standpoint? Obviously, there are not that many U.S. scientists who speak Japanese.

What can we do about this obstacle?

Professor BEASLEY. Let me just make one remark. There is, I believe, an NSF program which will provide some language training for people who are willing to work in Japan. I think that is an excellent idea. I think anything that eases that burden of somebody going over there is highly desirable. But you can go and function without Japanese. I do not think we should use that as an excuse not to go and get started. It is another thing we need to do, and so on and so forth, but you can go and spend short periods of time there, profitably. I think even that is helpful.

Senator ROTH. Let me go back to another question I asked earlier, in regard to the view that Europe is falling behind. Is there any particular reason for why we should guard against this precedent? Or is that just a broad problem that Europe has had from being divided?

Dr. QUISENBERRY. Again, I think the point was made here earlier, Senator, that Europe may be falling behind in exploiting science. I think it is wrong to assume that Europe is falling behind in the conduct of basic science. In fact, I think some of the European universities, including those in England, are still producing some of the finest scientists around.

But they do have problems and I think it has to do with the collapse of infrastructure or the deterioration of infrastructure in some of the countries. I think there may be some lessons there that I need to think about before I talked about them.

Senator ROTH. Well, I would be interested now or at any time in the future to hear them. I think this is an extraordinarily difficult area for Government involvement. I am not persuaded Government is so finely tuned that it should get involved in trying to direct research.

Let me ask you this question. It goes to something you mentioned, Dr. Rowell, about the danger that much of our money could be wasted from the Government ending up with the private sector spending less.

Would any of you care to comment, are tax incentives a better means of trying to promote research in the private sector? Or would you have the same criticism?

Dr. ROWELL. I do not think I was necessarily saying that it would be wasted. My fear was, and I suspect this would happen at home, if I got a significant grant from one of your funding agencies, I think my owner companies would say, well, next year you do not need so much money. So I think they would take back some of those funds.

Now, I think there are cleverer ways in which tax—I am no expert on the way taxes stimulate companies to work—but, for example, at present I see companies paying a lot of taxes which then comes here to Washington and then you essentially hand it out to do research, say in a Federal lab or a university and so on.

Maybe there would be some way where instead of that tax completely disappearing and kind of being lost track of, that the industry would be stimulated to take that money, or a rebate on that tax, if you like, and put it more specifically in certain places, and I think if they felt ownership of that tax money, then they might try and make the result of the research more specific for them rather than kind of a more general result for the country as a whole.

I think if the industry was made to feel responsibility for the money and for funding, you would still have to somehow stimulate them to spend that much, so you cannot let them off the hook. But the money, if it went to a university, if I gave money to Mac, then I would make sure Mac gave me something in return.

Senator ROTH. Well, I regret that time does not permit us to continue. My concern is that I think research and development is the life blood of this country's future and unfortunately Government is much like the private company; it is easy to cut back on it because it does not seem to have any immediate impact.

Gentlemen, I appreciate very much your being here today. Your testimony is most helpful and if there are additional questions we will submit them to you.

Thank you very much.

At this time I would like to call forward Dr. William Graham, who is the Science Advisor to the President, and the Honorable Bruce Merrifield, who is the Assistant Secretary of Commerce for Productivity, Technology and Innovation.

Gentlemen, it is a pleasure to have both of you here. We will be happy to include your full statements.

TESTIMONY OF D. BRUCE MERRIFIELD, ASSISTANT SECRETARY OF COMMERCE FOR PRODUCTIVITY, TECHNOLOGY, AND INNOVATION [1]

Mr. MERRIFIELD. Thank you, Mr. Chairman. I would like to do that and just make a few comments.

First of all, I would like to suggest that the Jack Gibbons OTA report is an important model, a competitive assessment of current activities that is essential for appropriate discussion and debate about the options that might come from that, and that is often the missing link. I think that this makes a major contribution. Basically it is a model, also, for not only superconductivity but a whole bunch of other things that are going to be coming down the pike sooner or later.

I would like to distinguish between big science and big technology. Big science, includes things like fusion energy, space and ocean exploration, the superconducting supercollider, things where the

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[1] See p. 83 for Mr. Merrifield's prepared statement.

Government really has to do the funding because industry really cannot undertake things that have very long-term commercial potential way downstream.

However, there is another area—big technology—that is much more related to commercial operations, and these are things like Sematech, for example——

Senator ROTH. Like what?

Mr. MERRIFIELD. Like the Sematech consortium where without some Government shared cost funding that might not have happened or might not be in place yet. It has taken us about 5 years to get that thing started.

High-definition television is another good model of this where really if we get our act together we could probably recapture the technical-industrial leadership in a lot of consumer electronics businesses. The sequencing of the human genome, for example, might contribute enormously to business opportunities downstream and there may be a Government role in this sort of thing. Basically, the idea is that help is needed where individual companies cannot undertake alone the risk, the long-term negative cash flow, that is necessary to bring things to commercialization.

We have some marvelous examples, of course, like the VCR, which currently would account for something like $18 billion in sales and hundreds of thousands of jobs if we had developed it instead of the Japanese. We invented it. Color television first developed by RCA, is another good example of that. The point is that we do have to make some accommodation to these sorts of things and superconductivity is one of these examples. But as previously pointed out in earlier testimony, superconductivity is at a very early stage and collaborative efforts of any significance are not appropriate at this point.

However, for example, if we wave our magic wand and we have something close to room-temperature superconductivity with a million amps per square centimeter current density, we now have enormous potential for collaborative efforts. For example, we then could access remote energy, very cheap energy. For example, up in Prudhoe Bay we pump most of our natural gas back into the formation because there is no way to get it down here. Well now we might take the Argonne laboratory fuel cell and put it at the wellhead, convert the gas to electricity, run it down a DC superconducting cable, pump it into the spinning reserve of superconducting energy storage spinning reserve here in our grid, and what we have is not only access to energy now being wasted, but that also will allow utilities to run flat out instead of cycling up and down every 24 hours, adding maybe 20 percent to their capacity without the additional capital investment.

Now, that kind of investment, though, is going to take collaborative efforts among utilities and other agencies to put in that kind of system. That is the appropriate time for collaborative efforts and possibly government assistance in making something like that happen. But at the early stages of basic research where we are now, it is hard to justify that kind of collaboration.

I think, more appropriately, it is important that we mobilize the tremendous capabilities that we have by creating the climate for entrepreneurial activity that is essential, and I think we have in-comparable assets that are undermobilized. We spend $15 billion a year in basic research, which is many times more than any other Nation does or can do. It gives us access to the leading edge technology that is essential.

We have 15 million companies in this country that can translate new knowledge into useful things; the greatest depth and diversity that exists anywhere in the world; an entrepreneurial culture that provides permission to try and fail, try again, without personal or social penalty; 4.5 million trained scientists and engineers in this country, many more than any other country; the world's largest market, of course, with a common language.

Our basic problem is that we have significant barriers to this process and I think this is probably our key problem. One of the barriers, of course, is the high cost of capital in this country, two to three times higher here than in Japan or other countries. In fact, if you could really account accurately for the hidden subsidies in the Japanese process, they may have almost a negative cost of capital which allows them to take long-term investments without significant risk. I think that is the most important problem that we have.

For decades the real cost of capital in this country was 2 to 3 percent. Since about 1979 it has been 6 to 8 percent, a very high, very punitive approach for not only investments in innovation but also in the automated systems, the manufacturing systems that are needed downstream.

The anti-trust laws have been very anti-competitive. As you know, we did change the anti-trust laws to allow collaborative R&D. In fact, we have started up about 78 major consortia now, involving over 1,000 companies, since that law was passed. And that will make a major contribution downstream to our competitive situation.

However, we still need further modification of the anti-trust laws to allow collaborative manufacturing. Sematech is a marvelous example of this, again. They are going to be building a 16-megabyte flexible computer-integrated manufacturing facility, but guess what? They are not going to be allowed to operate it because the anti-trust laws do not allow that in manufacturing. That is ridiculous. We need to change those laws to allow that collaborative effort, and that is going to apply to superconductivity downstream in the systems approaches that come out of the eventual work.

The liability laws are a good example of a barrier to a lot of innovation. The process patent laws are another example. You see, much of the economics now involved not only in superconductivity but in all these other areas are going to be involved in the process technology. But our process patents allow anybody to pirate our process technology and use it against us, and this is an important thing that we need to change.

There are dozens of regulatory barriers that need selective modification to be less anti-competitive, and bureaucratic impediments in the technology transfer acts and so forth that need to be addressed.

Well, basically, what I am pointing out is that the major Government role here is to get these barriers out of the way and provide selective incentives for innovation and automation. The rubric of our time is "innovate, automate or evaporate," and I really think we need to address that problem.

But there is a process involved here, too, and I think that is the last thing I want to comment on here. And that is that it starts first with a competitive assessment, like the OTA study, that provides the database against which to discuss the options, the pros and cons and so forth. The second step, then, is the dissemination of that database, either through the National Technical Information Service or, preferably, through conferences that display the information and offer the opportunity then for consortia of companies to begin to form.

But the formation of those consortia also takes a catalytic function, and this is a Government role. Often companies feel constrained about talking to each other but in the context of a Government conference they can get together, and in fact this is exactly what has happened. Many of those 78 consortia that have formed since we got the cooperative R&D act through have been formed in my office. And without that catalytic function they would not have done that.

Finally, of course, is the possibility—and only the possibility—of shared cost funding with the Government if that is necessary. But each situation almost has to be addresed on an individual basis. I think we have an opportunity—a historically unprecedented opportunity—now to mobilize the incredible resources that we have that are unique in the world to recapture the technical and industrial leadership in almost any area of industry and business operation, but the important thing is to create that climate, get the barriers out of the way, provide incentives.

And I see that as a primary Government role, a non-interventionist role. It does not try to manage and direct things in the private sector but does remove barriers, does provide incentives and does catalyze the collaborative efforts, which increasingly will be necessary.

Thank you.

Senator BINGAMAN. Thank you. Dr. Graham?

TESTIMONY OF WILLIAM R. GRAHAM, JR., PH.D., SCIENCE ADVISOR TO THE PRESIDENT, DIRECTOR, OFFICE OF SCIENCE AND TECHNOLOGY POLICY

Dr. GRAHAM. Thank you very much, Senator Bingaman, Senator Roth. I appear here today as the Director of the Office of Science and Technology Policy and as such am pleased that you have invited me to testify on this important industrial, scientific and technological matter. I will submit my full remarks for the record,[1] if I may, and then summarize them more briefly.

The President recognized the importance of a basic discovery of high-temperature superconductivity in early 1987 and chose a course of action to ensure this Nation's leadership in this and other future technologies. We are now moving forward aggressively to complement superconductivity technology development and commercialization and I will describe some of the ways in which we are doing that today. That is not to say that we cannot find further means of doing it, and the OTA report I think gives us the basis for further discussion on how we might proceed and perhaps points out some pitfalls that we want to avoid, as well.

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[1] See p. 100

Of course, the Federal Government has been supporting research on superconductivity since the 1940s and this Administration remains committed to supporting low-temperature superconductivity research as that of the high-temperature superconductors. In fact, most of the support for the low-temperature superconductivity research is done in non-military functions within the Government, including the National Science Foundation, NASA, and the Department of Energy, and we view the support for low-temperature superconductivity as an essential foundation for our work on high-temperature materials.

The discovery of superconductivity at liquid nitrogen temperatures in January, 1987, had a great impact on the scientific world. The significance of that breakthrough went far beyond raising the critical temperature, and in fact was so important that the President has designated Dr. Paul Chu, the first individual to develop a superconductor at temperatures higher than liquid nitrogen, to be one of the recipients of the National Medal of Science for this year, and I hope he will forgive me, because I have not told him that yet, but I will let him know as soon as I leave here.

The profound effect was shattering on both the theoretical understanding of the phenomena itself and the temperature limits. If we could achieve superconductivity at liquid nitrogen temperatures, might we not be able to achieve it at room temperature as well? That is still a hope we have but the theoretical basis of the current phenomena, much less room-temperature superconductivity, is as yet unclear, and so we are conducting an active program of research in the theoretical areas as well as the experimental.

Of course, as a part of the Federal activities, we have mobilized a number of mechanisms within the Government and the larger private sector and the President instructed that we carry out a Federal conference on the commercial applications of high-temperature superconductivity, which was held just about a year ago, in July of 1987.

The President's superconductivity initiative, an 11-point program, was issued at that time specifying a number of concrete actions I will report on today. The conference was an assembly of about 1,400 individuals from American Government, industry and universities who have a direct interest in American competitiveness. Its purpose was aptly described by the President in his address when he said, "We are here at this conference so that business and science can cross-fertilize, can begin at these early stages to dream and plan together because this new age of superconductivity is a new era for the spirit of enterprise."

The conference in its purpose, and I believe in its accomplishment, was to alert our Nation's scientists, engineers and entrepreneurs of the opportunity at hand and the commitment that the Administration had to encourage the exploitation of superconductivity. Very much to the point, the private sector was also reminded that they had the ultimate responsibility to translate the basic research discoveries of this exciting new field into commercially viable products.

We believe that the private sector is responding, not only in the established laboratories of major companies, but with new ventures. Entrepreneurs are planting the seeds that will sprout into an innovative superconductivity industry. This is very much in the pattern of other U.S. high technology ventures and one that we think will be very productive. In fact, when I go back and think about areas of advanced technology introduced around the world in the commercial sector, virtually all of them were first introduced in the United States, not only as scientific discoveries but as commercial products.

And if today we find ourselves at a disadvantage in some areas of commercial high technology, I believe that we need not look as much at our basic science, or even at our process of developing these to the market in their first manifestation, their initial appearance on the market, as we do at the industrial and economic processes that continue past that initial introduction. Dr. Merrifield mentioned several examples of those and I will not recite them or go further, but they are all too familiar to us I am afraid.

In addressing the President's Initiative, let me mention a few of the activities that have occurred under that, some of the key actions, including the creation of the Superconductivity Materials Subcommittee of the Federal Coordinating Council for Science, Engineering and Technology's Committee on Materials, which was established and had its first meeting over a year ago.

Its purpose was to coordinate superconducting materials research being done by the agencies of the Federal Government to identify relevant issues and concerns and to bring them to the attention of the Committee on Materials and thence to the Federal Coordinating Council, which I chair and which has on it the senior technical members of each of the agencies.

They have had over six meetings to date and carried out an extensive program of coordination, which I think the greatest value from is the discussion and the perspective provided to each of the members of the activity going on in other agencies of this very large Federal Government. As a part of that, they have also prepared and periodically update a compendium of the Federal research programs in superconductivity, and if you wish, Mr. Chairman, I will be glad to provide you with a copy of that.[1]

Senator **Bingaman**. We would appreciate that.

Dr. **Graham**. One of the advantages of the American system is the flexibility it permits, both in Government and in industry, and we took advantage of that flexibility in the R&D funding when this breakthrough was made. The Federal system responded to the discovery of high-temperature superconductivity by redirecting funds to get programs underway in 1987 and in total that year we have accounted for almost $45 million directed toward high-temperature superconductivity.

Furthermore, in fiscal year 1988 that number will over double to almost $95 million and we project it to rise to $135 million in fiscal year 1989. In total, the U.S. Government has committed more funding and committed it more rapidly to research on superconductivity than has any other Nation, and I point out that, as you know only too well, this has been accomplished within the current severe Federal budget constraints. That is in part because the President has been a consistent supporter of increases for our scientific research and so each year has provided for a larger budget overall for basic research.

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[1] See p. 280.

Of course, an early product of that initiative was the designation of certain superconductivity centers within the Federal agencies and their laboratories. These centers are in place and actively developing the enabling technologies necessary to commercialize superconductivity and establishing and expanding the interface between the Federal laboratories, the universities, and particularly, our industries.

Also, under the direction of the President's Initiative, a superconductivity advisory group has been established to advise the Administration on research and commercialization policies related to superconductivity. The Chairman of the group is Dr. Ralph Gomory, IBM Senior Vice President for Science and Technology and a member of the White House Science Council. The Executive Director is Dr. Praveen Chaudhari of IBM, and the members include Dr. Kent Bowen of MIT, Dr. John Foster of TRW, Dr. Theodore Geballe of Stanford University, Mr. Kenneth Oshman, a private investor, and Dr. Robert Schrieffer of the University of California, the "S" in the BCS theory of low-temperature superconductivity. This group has met several times and will report their findings to me this summer and then I anticipate periodic updates thereafter.

Legislatively, we have proposed an initiative which we call the Superconductivity Competitiveness Act of 1988, proposed to the Congress in February of 1988. The Act proposes to amend the National Cooperative Research Act to cover joint production ventures, as Dr. Merrifield was describing a few moments ago. It also proposes to increase U.S. patent laws and protection for holders of process patents, and provides protection for certain commercially valuable science and technology information generated in Government-owned and operated laboratories, and in particular relieves Government scientists of compulsory obligations which now exist to disclose commercially valuable scientific data to basically anyone who asks for it, from this country or otherwise—a rather unique burden they bear, and one which as far as I know no other scientist in this country suffers under and no other scientist anywhere in a foreign country has to live with.

If I may say a few words about the international aspects of superconductivity and scientific cooperation, because they have weighed so heavily in our discussions this morning, certainly the Japanese have a focused and strong program in superconductivity. And it is also clear that their large corporations, as several of ours, have targeted the development of superconductivity technology, and the fact that they are working so hard at it because we are, if that is true—and I think it is probably only partially true—is a good thing, I think, and it is a good thing because it will also help make sure that our companies and our enterprises work to their greatest potential, which is, in fact, enormous in the technology areas.

Senator **Bingaman**. Dr. Graham, let me just interrupt for a moment, if I could. We are going to have to conclude the hearing about 12:30, if you could complete your comments. I did have one question I wanted to put to you before we had to finish.

Dr. **Graham**. By all means. Let me summarize very briefly then by saying that within the last year we have done two things to strengthen our scientific cooperation in science and technology internationally while at the same time providing for the protection of intellectual property rights and other matters which underlie the commercial benefit of the research and development that we do in this country.

The first is the establishment of a framework for cooperation in science that I proposed to the Organization of Economic Cooperation and Development, a 24-nation consortium that was a derivative of the Marshall Plan. I proposed it in October of 1987 and it moved at record speed through the OECD machinery and was finally formalized as an overall OECD recommendation at the senior ministerial meeting in May of this year.

We have also just completed and the President has just signed a science and technology cooperation agreement with Japan, which we had in negotiation for about a year and which, among other things, I think is quite trail-blazing for our research cooperation with many countries. It will provide for cooperation in all areas of Government-supported research, including that which is done in industrial laboratories as well as universities. In a sense, we look forward to working with the Japanese where they do their best research, just as they work with our with us where we do our best research, much of it in our universities.

I will conclude by making a couple of comments about the OTA report. I certainly concur with their favoring increased funding of the National Science Foundation and find that these proposals follow the recommendations this Administration has argued for frequently, including in the last budget cycle and in this one. The fact that we requested a 17 percent increase this year for NSF and were able to obtain only a 6 percent increase in the appropriation was, I am sure, as great a dismay to us in the Administration as it was to the Office of Technical Assessment, and all their support in this area would be most appreciated.

Furthermore, we think the merit of the university and the United States organizational system is very strong, but I must take exception with some of the aspects of their report. In particular, I believe their alternatives that go beyond the existing Administration policy of a complementary role for Government, universities and industry is ill-cast. In fact, I think it is a solution which would create the problem that they so much avoid to bring upon us, and the question of why the Europeans are more or less—or I would say, a lesser player in this field, compared to the United States and Japan, I think in large part can be traced back to just this issue.

Europe has a tradition of a Government intruding itself or involving itself heavily in product-oriented development and I believe, as several of your previous witnesses have, that that has a debilitating effect on the industry while taking from the Federal budget funds which are much better spent in organizations like the NSF and the agencies for their basic research work, and I find that the proposal of starting a Department of Technology or a Technology Agency to be equally counterproductive. I personally, and I believe this Administration, would much rather see those funds directed to the NSF and other research functions which the Government does so well, and not have the Government intrude in commercial and market decisions for which it has neither the incentives nor the experience to contribute.

So on balance, I wish that my friend, Dr. Gibbons of OTA and his staff had talked with me or other senior members of OSTP as the report was being prepared. But in any case, I look forward to working with Congress in assessing the impact of some of the recommendations and in developing future legislation.

Thank you.

Senator BINGAMAN. Thank you very much.

Let me just ask one question. I gather from what you are saying that you and the Administration do not support the recommendation of establishing a working group on commercialization which the report recommends?

Dr. GRAHAM. Well, in terms of the option two reports, I believe we have done certainly the most productive part of those and we have established what I think is a very effective group. The one that I mentioned, chaired by Dr. Gomory, is specifically designated by the President in his Initiative to provide review and recommendations to the Federal Government.

Senator BINGAMAN. This is your advisory group that you referred to?

Dr. GRAHAM. Yes, the six-member group.

Senator BINGAMAN. But that group focuses on the research rather than on the commercialization, as I understand it.

Dr. GRAHAM. No. Of course, these are not divorced issues, but it is both well-membered and I believe well-prepared to provide recommendations to the Government across the broad range of Government involvement in superconductivity. And since each of these gentlemen represents a distinguished organization himself, either university or industrial, I believe their words will be listened to carefully by our academic and our industrial sector as well. So we look to them for broad recommendations, not narrow.

Senator BINGAMAN. So you see this Superconductivity Advisory Group which you already have in place as fulfilling all the needs that we have with regard to central direction for this area?

Dr. GRAHAM. Well, I believe it fulfills the needs for a senior advisory group for both Government activities and commercial initiatives in this area. But I must tell you, I shrink from the idea of a centrally controlled organization being established to direct what we do in this country. I believe that is one of the problems Europe has in many areas and I see them trying to move away from it, and I think we should recognize that while you cannot draw our organizational structure on a simple line chart very easily, we as a country have learned to live with a great deal of diversity in what we do and we draw a lot of strength from it and it is managing that diversity that is the real challenge before us, and I think we do that better than any other country in the world.

Senator BINGAMAN. All right. I apologize for having to terminate things here but we have run out of time. I do appreciate your patience in waiting to testify and I appreciate your testimony very much.

Thank you.

Dr. GRAHAM. Thank you, Mr. Chairman.

Senator BINGAMAN. The Committee will stand in adjournment subject to the call of the Chair.

[Whereupon, at 12:30 p.m., the Committee adjourned subject to the call of the Chair.]

APPENDIX

Statement of The Honorable Dave McCurdy
before
The Senate Committee on Governmental Affairs
June 28, 1988

Mr. Chairman, it is my pleasure to appear before your Committee today to discuss high temperature superconductivity and its potential for commercialization. As you are undoubtedly aware, high temperature superconductivity has been the subject of numerous Congressional hearings, a number of which have been conducted by the House Committee on Science, Space, and Technology. The most recent was conducted by my colleague from Tennessee, Congresswoman Lloyd last week. It is my intention today to briefly outline what we have heard in our deliberations as well as to discuss legislative efforts that have been introduced by myself and Congressman Ritter of Pennsylvania. I believe much of what I say will dovetail nicely with the findings and recommendations of the OTA report you will officially receive this morning.

WHAT WE LEARNED AT THESE HEARINGS CONFIRMED MUCH OF WHAT WE HAD BEEN READING IN THE PRESS -- THAT THESE NEW MATERIALS OFFER THE POTENTIAL FOR VAST, REVOLUTIONARY CHANGES IN SUCH DIVERSE AREAS AS MICROELECTRONICS, TRANSPORTATION, COMMUNICATIONS, ELECTRICAL ENERGY, AND OTHERS. WHAT WAS NOT EMPHASIZED IN THE PRESS WAS THAT MUCH WORK STILL REMAINS. YEARS, POSSIBLY DECADES, OF EFFORT WILL BE NECESSARY TO USE THESE MATERIALS AT THEIR FULL POTENTIAL. THIS LONG-TERM RESEARCH EFFORT WILL REQUIRE A MAJOR LONG-TERM COMMITMENT OF FUNDS AND HUMAN RESOURCES. THE COST HAS BEEN ESTIMATED AT HUNDREDS OF MILLIONS OF DOLLARS ANNUALLY IN BOTH THE PRIVATE AND PUBLIC SECTOR.

OUR ANALYSIS OF FEDERAL SPENDING SUGGESTS THAT OVERALL R&D FUNDING FOR HIGH TEMPERATURE SUPERCONDUCTIVITY APPEARS MORE THAN ADEQUATE. THIS RESEARCH APPROACHED $90 MILLION IN FY 1988 AND WILL EXPAND TO MORE THAN $130 MILLION IN FY 1989. UNDERLYING THESE IMPRESSIVE NUMBERS, HOWEVER, WE SEE OTHER PROBLEMS. FOR INSTANCE:

- o NO NATIONAL PROGRAM EXISTS; INDEED IT IS HARD FOR US TO IDENTIFY EVEN INDIVIDUAL AGENCY PROGRAM PLANS, WITH DoD BEING A POSSIBLE EXCEPTION.

- o OVER TWO-THIRDS OF THE MONEY IS BEING DIRECTED THROUGH DoD;

- o ONLY MINIMAL AMOUNTS OF THE RESEARCH IS DIRECTED TO PROCESSING AND ENGINEERING SCIENCES, ESSENTIAL TO COMMERCIAL DEVELOPMENT OF THESE MATERIALS;

- o INDUSTRY HAS HAD LITTLE OPPORTUNITY TO PROVIDE FORMAL INPUT INTO CURRENT AND FUTURE RESEARCH PLANS;

- o WE HAVE NO FORMAL LONG-TERM COMMITMENT TO HIGH TEMPERATURE SUPERCONDUCTIVITY. DESPITE THE ENTHUSIASM SHOWN BY THIS ADMINISTRATION, THERE ARE NO GUARANTEES THAT THE NEXT ADMINISTRATION WILL BE EQUALLY WILLING TO PURSUE THIS WORK.

LET ME EXPAND BRIEFLY ON ONE OF THESE CONCERNS -- LACK OF A COMPREHENSIVE NATIONAL SUPERCONDUCTIVITY PROGRAM. IN LIEU OF A CLEAR DEFINITION OF PRIORITIES AND GOALS AT A NATIONAL, FEDERAL LEVEL, WE HAVE A MULTITUDE OF PRIORITIES AND GOALS, SOME ARTICULATED, BUT MOST NOT. THESE ARE DIRECTED, NOT SURPRISINGLY, TO THE INDIVIDUAL MISSIONS AS INTERPRETED BY INDIVIDUAL AGENCIES. THUS, THEY ALL SEEM TO BE HEADING IN DIFFERENT, INDEPENDENT DIRECTIONS. SOME ARGUE THAT DIVERSITY IS NECESSARY AND A NATIONAL PROGRAM WOULD INHIBIT FLEXIBILITY. BUT JUST AS OBVIOUSLY, TOO MUCH DIVERSITY LEADS TO CHAOS. PERHAPS, MORE IMPORTANTLY, A NATIONAL PROGRAM WOULD SEND A CLEAR SIGNAL TO OUR INDUSTRIES, SUCH THAT THEY COULD MOST EFFECTIVELY DEVELOP THEIR OWN PROGRAMS AND CORRELATE THEM APPROPRIATELY.

THE PROBLEMS CAUSED BY THE LACK OF A NATIONAL PROGRAM WITH CLEARLY DEFINED PRIORITIES, GOALS, AND COMMITMENT CAN BE, PERHAPS, BEST ILLUSTRATED BY RECENT ACTIONS IN DoD. DEPUTY SECRETARY OF DEFENSE TAFT, IN CITING OVERRUNS IN DEFENSE OUTLAYS, THIS PAST MONTH IMPOSED A SPENDING FREEZE ON ALL "NONESSENTIAL" DoD ACTIVITIES. THIS INCLUDED WINDOW WASHING AND LAWN MOWING AS WELL AS ALL NEW RESEARCH AND DEVELOPMENT. UNFORTUNATELY, DARPA, ALTHOUGH HAVING AWARDED $20 MILLION TO 35 CONTRACTORS AS PART OF A PRESIDENTIAL INITIATIVE IN HIGH TEMPERATURE SUPERCONDUCTIVITY, WAS ABLE TO FINALIZE ONLY TWO OF THESE CONTRACTS BEFORE THE IMPOSITION OF THE FREEZE. EXTENSION OF THE FREEZE BEYOND JULY 1 WILL PROVE DISASTROUS FOR MOST OF THESE SMALL, ENTREPRENEURIAL COMPANIES. SEVERAL IN FACT, FACED WITH EXTINCTION, MAY FIND IT EXTREMELY DIFFICULT NOT TO SUCCUMB TO FOREIGN PRESSURE TO SELL THEIR TECHNOLOGY ABROAD.

A NATIONAL PROGRAM AND COMMITMENT CANNOT GUARANTEE AGAINST A REOCCURRENCE OF SUCH A SITUATION. HOWEVER, I BELIEVE THE EXISTENCE OF SUCH A PROGRAM, TOGETHER WITH A CLEARLY ENUNCIATED PRESIDENTIAL AND CONGRESSIONAL COMMITMENT, WOULD CERTAINLY MINIMIZE SUCH PROBLEMS WITHIN INDIVIDUAL AGENCIES.

MR. CHAIRMAN, LET ME BRIEFLY MENTION LEGISLATION THAT CONGRESSMAN RITTER AND I, TOGETHER WITH A NUMBER OF OTHER MEMBERS, BELIEVE WILL ADDRESS A NUMBER OF CONCERNS I HAVE OUTLINED HERE TODAY. A MEASURE, DEVELOPED BY MR. RITTER AND MYSELF, TENTATIVELY ENTITLED THE "NATIONAL SUPERCONDUCTIVITY, COMPETITIVENESS AND NATIONAL SECURITY ACT OF 1988" HAS THE FOLLOWING MAJOR THRUSTS:

(1) IT CALLS FOR THE ESTABLISHMENT OF A NATIONAL FEDERAL PROGRAM FOR ADVANCED SUPERCONDUCTIVITY R&D, TO BE DEVELOPED THROUGH THE EXECUTIVE OFFICE OF THE PRESIDENT.

(2) IT WOULD STRONGLY INVOLVE THE PRIVATE SECTOR IN DEVELOPING THE NATIONAL PROGRAM, PRIMARILY THROUGH A PRESIDENTIAL NATIONAL COMMISSION ON SUPERCONDUCTIVITY (CREATED BY THE TRADE BILL).

(3) LONG-TERM COMMITMENT TO THE PROGRAM WOULD BE PROVIDED THROUGH THE AUTHORIZATION OF BASE FUNDING OF $120 MILLION PER YEAR FOR A PERIOD OF FIVE YEARS.

(4. RESEARCH RESPONSIBILITIES ARE BROADLY OUTLINED FOR DOD, DOE, NSF AND NBS, WITH PARTICULAR FOCUS ON THE ROLE OF DARPA IN PROCESSING THESE MATERIALS.

IN CLOSING, MR. CHAIRMAN, LET ME NOTE THAT EVEN WITH THE ESTABLISHMENT OF A NATIONAL PROGRAM AS I'VE OUTLINED HERE, IT IS NOT OBVIOUS THAT OUR NATION WILL SUCCEED IN COMPETITIVELY DEVELOPING AND APPLYING SUPERCONDUCTIVITY. HOWEVER, I BELIEVE SUCH A NATIONAL PROGRAM IS ESSENTIAL IF WE ARE TO HAVE A CHANCE OF SUCCESS. I LOOK FORWARD TO WORKING WITH YOU AND OTHER INTERESTED MEMBERS IN HELPING ESTABLISH SUCH A PROGRAM.

THANK YOU MR. CHAIRMAN. I'D BE HAPPY TO ANSWER ANY OF YOUR QUESTIONS.

STATEMENT OF

JOHN H. GIBBONS

DIRECTOR, OFFICE OF TECHNOLOGY ASSESSMENT

BEFORE THE

COMMITTEE ON GOVERNMENTAL AFFAIRS

UNITED STATES SENATE

JUNE 28, 1988

Mr. Chairman and Senators, OTA is pleased that the Committee on Governmental Affairs is releasing Commercializing High-Temperature Superconductivity at this hearing. With me are John A. Alic, the project director, and W. Wendell Fletcher, deputy project director.

The Senate Committees on Energy and Natural Resources, and Commerce, Science, and Transportation, along with the House Committee on Science, Space, and Technology, also requested this assessment. The four committees asked OTA to look, not only at commercialization, which we report on today, but also at the longer-term R&D agenda for this exciting new technology, and at prospective applications -- the subjects of a second report, now underway in the OTA Energy and Materials Program. Given the speed of the scientific advances over the past year and a half, a good deal will likely change before the second OTA report is completed, next year.

I might also mention that OTA has conducted many other assessments dealing with related topics. I've attached a list of the more recent reports to this statement.

I will begin by outlining why high-temperature superconductivity (HTS) is so important, then go on to summarize OTA's findings, and the policy alternatives that stem from them. Our report includes 20 policy options -- many of them suggesting quite specific steps that the Federal Government could take to stimulate commercialization of HTS. They cover such matters as: budgets for HTS R&D; technology transfer from Federal laboratories to industry; and cooperation with Japan in HTS research. Some go well beyond superconductivity, since the requesting committees asked that we examine HTS in the broader context of U.S. technology policy.

The report also analyzes three strategies for commercialization: (1) the current approach, which relies heavily on R&D funding channeled through the mission agencies (primarily the Departments of Defense and Energy); (2) a more aggressive strategy, which would give greater support to commercial, non-defense R&D in superconductivity; and (3) organizational steps addressing the more general problems U.S. industry faces in technology development.

THE COMPETITION

The discovery of superconductivity at record high temperatures won perhaps the quickest Nobel Prize in history for two scientists at IBM's Zurich research laboratory. The science community did more than acclaim the discoveries by Müller and Bednorz; people in many parts of the world began working to push the temperatures further upward.

They succeeded. Progress over the last year and a half has been extraordinarily rapid -- and not only in the United States. Moreover, the scientific race is becoming a race to invention and commercialization -- a contest in which the primary competitors will be American and Japanese firms (Europe, despite unquestioned excellence in science and engineering, will probably not be able to keep up.)

Over the next decade, the Federal Government will bear much of the burden for supporting the HTS technology base. The competitive success of the United States vis a vis Japan will depend in part on how much money Federal agencies have for HTS, and how wisely they spend it.

American corporations have been backing away from long-term, high-risk R&D since the 1970s. This makes Government support for research -- in universities, in the national laboratories, in industry -- more important than ever for economic competition. Yet the United States has no tradition or policy for directly supporting commercial technology development.

The Japanese Challenge

More Japanese than American companies are at work on HTS. They have greater numbers of people looking at applications, and asking how HTS fits into business plans. When it comes to related technologies like ceramics and microelectronics, Japanese companies have amply proved their skills. They will be formidable competitors in HTS.

Moreover, while Japan remains generally behind the West in basic research, Japanese scientists have an excellent record in superconductivity. Government laboratories and universities, as well as major corporations, have substantial experience with the older generation of superconducting technologies -- in part because Japan has given superconductivity high priority because of its energy saving potential, highly important for a country with no fossil fuel reserves.

Perhaps most important, many government officials and corporate managers in Japan see in HTS the opening of a new era in international competition. To Japan, this is a chance to take the lead in high technology.

Comparing U.S. and Japanese Efforts

For many American managers, the uncertainty surrounding HTS urges caution. The Japanese are more optimistic. They believe investments now will pay off, some time and in some way. It may be a cliche to say that Japanese firms put more weight on growth and market share than on short-term profits, but it is true, and it is making a difference in this technology:

- According to OTA's surveys, Japanese companies have substantially more people at work on superconductivity than American firms, and are probably spending more money.
- The breadth of the Japanese effort exceeds that in the United States: more companies, in more lines of business are pursuing HTS R&D -- steelmakers and glass companies, for example.
- Most of the effort in the United States is going toward research. American managers believe HTS should remain in the laboratory until more scientific knowledge is in hand.
- Japanese companies are conducting research but also exploring applications. They trying to think through what HTS means for competitive strategy.

Managers in Japan see HTS as a means of creating new businesses, while American managers are more likely to view it in a context of their existing business. A number of larger American companies believe that if HTS takes off, they will be able to catch up or buy in. Japanese managers believe that advantages established now will last.

Taken as a whole, the U.S. approach -- driven by the need to show financial paybacks in the short term -- could leave American industry behind Japan within a few years. Indeed, American firms already seem to be falling behind.

POLICY IMPLICATIONS

Findings

Let me now turn to several of OTA's findings as they bear on Government policies:

1. During 1988, the Federal Government will spend about $95 million on HTS R&D -- about as much as industry, and a total that comes close to the $100 million recommended last year by a panel of the National Academy of Sciences. But, just as most of the Federal R&D budget goes for national defense, the Department of Defense (DoD) will have more for HTS R&D than any other agency. The Department of Energy (DOE) is next. These two mission agencies, together, have about three-quarters of the Federal money. Relatively speaking, the Nation's universities have not been getting very much for HTS research, nor have companies aiming at civilian, commercial markets. More money for commercial (non-defense) R&D in superconductivity would raise the odds on competitive success by U.S.-based firms. OTA's report discusses a range of alternatives for channeling R&D funding to support commercialization more effectively.

2. The money spent by DoD and the other mission agencies will help build a technology base that U.S. companies can capitalize on. Processing R&D sponsored by the Defense Advanced Research Projects Agency (DARPA) could be particularly important. The new HTS materials are ceramics, brittle and difficult to work with. Processing and fabrication will be critical for commercialization. DOE R&D, likewise, will feed the overall technology base. But technology transfer from DOE's laboratories to industry has not worked very well in the past (although the laboratories are trying to establish more productive working relationships with industry), while most of DoD's technology transfer experience involves defense contractors. Better mechanisms for technology transfer will be needed if U.S. industry is to take maximum advantage of Federal R&D spending.

3. With 5 to 10 years of R&D ahead, consistency and continuity in Federal support for HTS -- rather than the stop-and-go policies common in the past -- will be needed so that Federal agencies can plan ahead, along with industry and university research groups. The budget impasse between Congress and the White House last fall meant that no one knew until the 1988 fiscal year was well underway how much money there would be for superconductivity. At a time

when new research results were coming in at a rapid pace, confusion and delays in R&D funding made it difficult for the United States to keep up.

4. In HTS, as in microelectronics or ceramics, the problems faced by American companies will be matters of technology rather than science. The United States may make many of the discoveries in HTS, and American firms may develop many of the innovative new products, while Japanese companies nonetheless reap the rewards because of their skills in engineering, manufacturing, and marketing. Federal policies that steer more resources to engineering and technology, without sacrificing the potent advantages to the U.S. economy from leadership in science, could help American industry compete -- not only in HTS, but in many other fields. Again, OTA's report discusses a number of specific alternatives for accomplishing this.

5. Traditionally, U.S. technology policy relies on R&D financed by the mission agencies, which support projects targeted on their own needs. But gaps and holes sometimes open in the Nation's technology base where no agency has an immediate need, and the technical community views the work as less than glamorous. Manufacturing processes have been a prime example. Some of this could happen in superconductivity; if we wait until industry reacts to the problems -- as the semiconductor industry reacted in proposing Sematech -- Japan could already be well ahead. A proactive policy, channeling support to joint industrial R&D, proposed and carried out by private-sector consortia, could help avoid such outcomes.

6. In part because of pressure from the West, Japan has offered to open government-supported research projects in HTS to foreign participation. This is not altruism on the part of the Japanese; their research system, particularly in universities, is inbred and often stifling. The Japanese think foreign participation could bring in fresh thinking and creative new ideas. But the United States has just as much to gain. Only by participating side by side with Japanese engineers and scientists will Americans come to understand how Japan competes so effectively through technology. Policy options here include language training and fellowships for American engineers and scientists, so they can engage in hands-on R&D within Japanese laboratories.

While important, policy options dealing with such issues as management of the R&D budget leave aside questions of the overall direction of the Federal effort, for HTS and for technology policy generally -- questions of strategy.

Strategies for Commercialization

The U.S. Government moved quickly after the initial breakthroughs in superconductivity to redirect R&D money. Funding for HTS doubled from $48 million in fiscal 1987 to $95 million this year -- demonstrating the responsiveness of the system. Yet, as I've already noted, there are real reasons for concern -- notably, lack of money flowing to industry, and heavy *de facto* reliance on DoD and DOE.

Strategy 1: Flexible Response. The first of the three strategies OTA analyzes -- a continuation of the traditional approach -- implies the following:

- Heavy ongoing funding for defense applications of HTS. Although DARPA, for example, has stated that the results of its processing contracts will remain unclassified, such a policy would be subject to change, depending on outcomes.
- DOE would pursue its own mission-oriented projects, with most of the money going to the national laboratories, while the executive branch continued its efforts to open up the national laboratories.
- The Administration would also continue to press for relaxed antitrust enforcement and stronger intellectual property protection, seeking greater incentives for private-sector investments in R&D and technology development.
- Some State Governments would channel support, direct and indirect, to HTS as part of technology-based economic development programs.

Will such a response, by itself, be enough to keep up with our international rivals? OTA's analysis indicates that it will not.

The U.S. effort looks formidable in the middle of 1988, but that is to be expected: the challenges so far have been largely matters of science. If there is a surprise, it is that the Japanese -- not in the past known for innovation in science -- have already posted such a strong showing in HTS.

The strengths and weaknesses that have repeatedly characterized U.S. performance in high technology are already on display in this technology. The issue is not overall funding levels. The issue is the allocation of those funds: mission-oriented R&D, coupled with basic research, does not provide enough support for commercialization to ensure that American firms will be able to keep up with their foreign competitors. In short, if the United States continues down the path we have started on, the widely expressed fear that Americans will win in science, while the Japanese take the commercial markets, could easily come true.

Strategy 2: An Aggressive Response to HTS. This approach adds three new elements to Federal Government activities already underway:

- A larger role for the National Science Foundation (NSF), both in funding individual research in HTS at universities, and through the establishment of one or more multidisciplinary university centers. Although basic research in HTS does not seem seriously underfunded, more money for NSF -- perhaps $20 million over a 5-year period -- would guard against missed opportunities in relatively fundamental work, of the sort few U.S. corporations undertake.

- Federal cost-sharing of long-term, high-risk HTS R&D planned and conducted by industry consortia (like the work DARPA supports for the military). This is the simplest and quickest way to steer more resources into applied research and generic technology development -- to projects industry views with favor, but with benefits that would be difficult for individual firms to capture, or that are well in the future.

- A working group on commercialization -- composed of experts drawn from industry, Government, and the universities. This body would be charged with helping shape consensus on the HTS R&D agenda, and making decisions on Federal financing for private-sector consortia.

This second strategy preserves the strengths of the traditional U.S. approach to technology development, while compensating for the weaknesses revealed over the past 15 years. It skirts the "picking winners" problems raised by Federal subsidies for private R&D by, in effect, allocating government resources to the highest bidders. Among otherwise qualifying R&D consortia, funds would go to those with the longest time horizons and greatest fraction of total costs borne by the corporate members. The aggressive strategy for commercializing HTS could substantially improve prospects for rapid commercialization of HTS and do so at relatively modest cost to the Federal Government.

Strategy 3: **A Federal Technology Agency**. Under this strategy, OTA addresses prospects for a new or reorganized agency charged with supporting commercial technologies. A perennial issue in U.S. science and technology policy, many alternatives have been proposed over the years. The possibilities range from a small, independent agency with an initial budget of less than $100 million, to a cabinet-level Department of Science and Technology pulling together some (though hardly all) of the R&D activities of existing agencies.

The cabinet-level Department of Science and Technology became the lead recommendation of the Young Commission on competitiveness in 1985, but found little support. Less ambitious proposals -- e.g., for a national technology foundation, paralleling NSF's role in support of science, or a civilian version of DARPA -- have also been frequent. The latter path has seemed particularly attractive, given DARPA's enviable reputation -- a small group of creative people, with the judgment and experience to seek out and support the best ideas. But DARPA has a mission, and a critical one -- support of long-term R&D with potentially big payoffs in military systems.

Lack of a comparable mission makes it difficult, though not impossible, to envision an agency that could succeed in supporting industrial technologies. Absent a clear-cut mission to lend discipline to processes for setting priorities and making funding decisions, a Federal technology agency could easily end up subsidizing marginal projects, while having little impact on the course of international competition. If such an agency is to support R&D in the public interest, it would need to find ways of identifying what that public interest is, convincing potential critics that it has done so fairly, and that the results justify continuing support. OTA's report offers a number of suggestions for tackling these problems.

In none of its forms could this third strategy offer support for HTS in the critical years immediately ahead, simply because of the time needed to put new mechanisms in place. But if too late for HTS, policy changes now could put the United States in a better position when the next opportunity like it comes along.

CONCLUSION

The three strategies OTA has analyzed are not exclusive, but they do represent differing views of the strengths and weaknesses of the U.S. approach to technology development. Advocates of the first strategy believe that the fundamental strengths of our system remain intact, and that industry will be able to commercialize HTS when the time is right.

Those favoring a more aggressive policy stress the dangers of a business-as-usual mentality, given the speed with which U.S. industry has lost its earlier advantages in high technology. The underlying worries over loss of technological advantage lead some to go further, arguing that the United States needs to thoroughly overhaul its approach to technology policy.

Support for science and for military technology served the United States well from 1950 to the middle 1970s. But the lesson of the past 15 years is plain: in a world of increasingly strong rivals, these levers no longer suffice. And it is not only Japan and West Germany that have been catching up. The newly-industrializing countries have made rapid strides; firms based in nations like South Korea and Taiwan have become able competitors in industries ranging from steel to electronics to construction. As they put pressure on Japan from below, Japan moves further into high technology.

Unless Congress and the Administration begin to explore new approaches, seeking to learn what will work for the United States, we will continue losing our technological advantages, and risk missing out when new windows of opportunity open. HTS is one of those rare opportunities, appearing every decade or so, that opens a new round of competition. Firms and nations that establish a strong position early can often keep the lead for years.

The United States retains many sources of advantage in international competition -- unmatched capabilities in science, an economy that is the largest, most diverse, most robust the world has ever seen. But the traditional U.S. approach to technology development -- heavy Federal R&D spending, coupled with indirect encouragement for innovation and technology development through the tax system, patent protection, an antitrust climate that seeks to encourage cooperative R&D -- no longer seems adequate. It is not too late to craft a policy that will help American industry in the coming competition in HTS. But by this time next year, it could be.

Attachment

OTA REPORTS DEALING WITH TECHNOLOGY POLICY AND COMPETITIVENESS

Technology and Steel Industry Competitiveness (1980)

U.S. Industrial Competitiveness: A Comparison of Steel, Electronics, and Automobiles (1981)

International Competitiveness in Electronics (1983)

Commercial Biotechnology: An International Analysis (1984)

Computerized Manufacturing Automation: Employment, Education, and the Workplace (1984)

Information Technology R&D (1985)

Trade in Services: Exports and Foreign Revenues (1986)

International Competition in Services (1987)

The Defense Technology Base: Introduction and Overview (1988)

Technology and the American Economic Transition (1988)

Educating Engineers and Scientists: Grade School to Grad School (1988)

Advanced Materials by Design (1988)

Commercializing High-Temperature Superconductivity (1988)

Paying the Bill: Manufacturing and America's Trade Deficit (1988)

U.S. Investment in Biotechnology (to be released July 1988)

Technology, Innovation, and U.S. Trade (ongoing)

RESPONSES TO QUESTIONS FROM SENATOR JOHN GLENN

FOR

DR. JOHN H. GIBBONS

Hearing before the Senate Governmental Affairs Committee
U.S. Efforts to Commercialize Superconductivity
June 28, 1988

Question 1: **What is the evidence for OTA's conclusion that "commercialization of HTS will proceed somewhat faster than American managers anticipate, though not so fast as many in Japan expect"?**

This conclusion reflects a judgement of the OTA project staff. Opinions among American managers and technical specialists on the likely pace of commercialization of HTS span a considerable range -- as revealed both through interviews and the published literature. (Managers in small entrepreneurial companies, not surprisingly, were typically much more positive about prospects for rapid commercialization.)

In interviews, most Japanese managers expressed substantially more optimism than Americans. In OTA's judgement, some of views expressed by Japanese managers are unrealistic -- in a few cases wildly so. At the same time, the financial pressures on many American managers push them in the direction of pessimism: errors on that side are easier to justify or excuse. Moreover, some American scientists now in R&D management positions have spent many years working with the older generation of low-temperature superconductors; they have learned to be cautious when it comes to technologies that the media tends to glamorize and oversell.

OTA states elsewhere in its report that some of the major impacts of HTS will be those that cannot now be anticipated. Other technologies -- e.g., the laser -- give ample evidence of the unexpected ways in which new developments come into use. Given the rapid progress in the science of superconductivity over the past two years, OTA expects relatively rapid commercialization. The first applications, of course, will be highly specialized.

Question 2: **Given the possible margins for errors in OTA's surveys of industrial R&D in HTS, how much confidence should be placed in conclusions based on these surveys?**

OTA is quite confident of its general conclusions concerning industrial R&D strategies in the United States and Japan, conclusions with a basis that extends well beyond the surveys. OTA's interviews in both countries are consistent with the survey results. So are the contents of dozens of articles in the business and trade press in both the United States and Japan. Finally, there have been several other surveys of Japanese scientists and businessmen (carried out by agencies of Japan's government and by the press, some translated, some available only in Japanese) that bear out the findings of the survey conducted for OTA by the Tokyo Office of the U.S. National Science Foundation. Thus, although the quantitative results of OTA's surveys are subject to error, as our report emphasizes -- and although industrial R&D in HTS is a moving target -- OTA's findings concerning business strategies in the two countries can be regarded as firm.

Question 3: Given the demands on the Nation's science and technology budget, compare HTS with alternative Federal R&D investments -- in particular, human genome mapping, the superconductivity supercollider, or the space station.

OTA finds no reason to criticize the magnitude of Federal R&D spending on HTS. The problem is best regarded as one of allocation. In particular, some of the money going to the Department of Energy -- which seeks some $39 million for HTS in fiscal 1989 -- might better be spent elsewhere, given that DOE has yet to demonstrate that it can effectively support commercialization of new technologies like HTS.

Beyond this, the needs revealed by OTA's analysis -- a matter of another $20-$30 million annually -- seem modest compared to projects such as mapping the genome, or building a space station or supercollider. Under OTA's Strategy 2: An Aggressive Response to HTS, additional funds would go to NSF for university research ($4 or 5 million per year) and for R&D proposed by industry consortia, as authorized by the expert working group established under this strategy (say, $25 million annually).

Perhaps the essential point is this. Hundreds of universities, companies, and government laboratories are carrying out R&D on the new materials. Thousands of scientists and engineers in a broad range of settings are making individual choices of research direction. In HTS, there can be no single project like the supercollider, while the research goals are far more various than involved in the genome mapping debate. Moreover, the supercollider would apply existing technology for probing the basic nature of matter -- with no thought or hope of direct technological spinoff. (For the genome project, commercial ideas *would* follow more or less directly from the research, but the scientific challenges come first.) For HTS, commercialization is a primary goal; the problem is not one of finding enough money to attack a single large research problem (genome mapping), or to build a facility like the supercollider with well-defined requirements (hence budgetary needs). Rather, the problem in HTS is one of ensuring that nothing vital falls through the cracks -- through oversight or lack of money -- so that Federally-funded R&D adequately supports timely commercialization by American firms.

Statement To

Senate Committee on Governmental Affairs
Hearing of Office of Technology Assessment's
Strategies of Commercialization of High-Temperature Superconductivity

by

R. K. Quisenberry
Director of Research
Central Research & Development Department
E. I. du Pont de Nemours & Co., Inc.

June 28, 1988

Good morning. I am Richard Quisenberry, Director of Research for Du Pont's Central Research & Development Department, in Wilmington, Delaware. At Du Pont, responsibility for and management of R&D is distributed over a wide range of business and technology interests. Work on high-temperature superconductors is in our corporate laboratory for which I am responsible. This corporate laboratory is the largest and – we hope – one of the most productive basic research units in the chemical and materials industries. Its mission is to support the long-range technology interests of Du Pont and to develop applications – including initial commercialization – for new technology. We have a tradition of basic research, application development and successful commercialization of materials of many kinds - including plastics, fibers, films, ceramics, imaging technology and materials for electronics – and recently biotechnology.

High-temperature superconductors appear to us to fit the mission of the corporate laboratory and accordingly we have mounted an effort which we believe to be the most substantial among the world chemical companies.

This morning I would like to take a few minutes and outline for you the scope of our program. I'll indicate how we have organized and why, and, along the way, offer a few observations on the report by the Office of Technology Assessment that was issued this morning.

I intend to make my remarks brief in the real hope of having time to discuss things that are on the Committee's mind.

The research and development program in high-temperature superconductors at Du Pont has three components:

1. Basic studies on the nature of the materials
2. Demonstration and early market testing of potential applications
3. Product planning and market evaluation

The OTA report also characterizes these three elements as hallmarks of the "Japanese approach to new materials." These have also always been characteristic of successful product introduction from the results of basic science in our company and in others. I know of no other way to do it. The notion of "monitoring" a fast-moving field – then jumping in at the right time – doesn't work. Such a strategy may be successful in a mature technology, but certainly not in this field.

In our basic research organization, we have broad scientific expertise and facilities in chemistry, ceramics, product characterization and solid state physics. These technology elements have been put in place over many years to provide materials to the electronics industry, pigments to the coatings, paper and textile industries around the world and for discovery of new catalysts for more effective synthesis of chemicals. Within this effort had been a group working on new superconductor materials. Within days of the first successful reports of high-temperature performance in January 1987, we were able to redirect things and mobilize an interdisciplinary task force to understand and extend the results that were being reported from Zurich, Tokyo and Houston. We have been among the first to identify accurately the unique physical structures which account for superconductivity in all the various chemical classes thus far discovered. We have developed attractive chemical syntheses to the important materials. We have in operation a semiworks facility to prepare high-purity material for our own research and that of collaborators in other laboratories.

As the OTA Report correctly points out, the important commercialization step of high-temperature superconductors will not be of the materials themselves but of their applications. Here again, many of the applications which have been visualized for these materials exploit manufacturing and processing capabilities in place at Du Pont for other products. To mention a few:

- Shaped articles in the form of wires. We believe we can draw substantially on our years of development of ceramic fibers. Similarly, our experience with ceramic-based composites is proving quite useful in seeking applications.
- Synthesis and management of the hard-to-manage precursors of the superconductivity materials.
- We have a broad background in laying down layers of materials for electronic applications (so-called thick-film and thin-film technology).

For these above reasons, we feel comfortable in justifying a long-range basic science program. In this effort, we have established collaborations with scientists at important universities, such as, Houston, MIT, Stanford, California-Berkeley, California-Santa Barbara, CalTech and Virginia.

With regard to the second element – product development, – experience with other new and evolving materials teaches us that, despite the most thorough product planning and analysis, the market place contains many surprises. The sooner we test the marketplace the faster we learn and can move ahead. Thus, it is critical to test conceptual applications for high-temperature superconductors with potential users and customers as early as possible. We know that some of these early trials will fail commercially – others will succeed. Again, as the OTA report states, this is also a well-used concept of Japanese high tech industry. To this end, we have established at our Experimental Station in Wilmington an applications concept facility for high-temperature superconductor applications. It is staffed by scientists and engineers with extensive and varied backgrounds in materials, processing technology and electronics applications. Its purpose is to enable any scientist at Du Pont with a reasonably good idea for superconductivity applications to come forward and test his concept with the aid and guidance of experts. Such an approach is novel in our Company. Its objective is to speed technology transfer from the concept to the demonstration stage.

To return to the OTA report for a moment, I would observe that the overriding criticism of the American industry in the Report seems to me to be that technology transfer to the commercial sector from our basic laboratories – be they in industry, academia or government – has been slow and unsatisfactory. If this is the conclusion of the report, I concur. Better ways to effect technology transfer between ourselves and outside laboratories is a major item on the agenda for research management at Du Pont. There are formidable barriers to such technology transfer – beyond the well-publicized "not relevant here" excuse.

Academia and government laboratories have traditionally not had a technology transfer mission. Scientists in these laboratories often have an understandably unrealistic view as to what it takes to commercialize something. We find that if we wait for proposals to be advanced by these laboratories, they are often late or lacking in scope. We conclude we must put mechanisms in place to extract technology from these laboratories in a more timely way. In so doing, we encounter a maze of university policies, indifference and, in some cases, restrictions associated with government funding already in place there. None of these is impossible to overcome; but they slow the process.

The final element of our program – product planning and business development – involves a third group of scientists who also have substantial business experience. They do three things: They define and quantify business opportunities; they identify potential development partners; and, they help set our priorities.

It is clear to us that if we are to be commercially successful with high-temperature superconductivity, we must develop research partnerships with others and ultimately commercial relationships. We are well on our way to forming many such relationships. Some are with large electronics and computer companies. Others are with companies with experience in imaging technology and still others are with small so-called start-up firms with scientifically intriguing, but high-risk, applications. We also belong to consortia. We belong to the industrial MCC superconductivity consortium in Austin, Texas. Through our Japanese subsidiary, we belong to MITI consortium described in the OTA report. We belong to CSAC here in Washington – Dr. Edward Mead of our staff, who also served on the review panel of the OTA study, is on the board of CSAC. We are finalizing plans to affiliate with several university-centered consortia.

Our program this year is budgeted for about $10 million. All of this is underwritten as long-term corporate research and is not assessed as a cost on the bottom line of any business manager in our Company. I note that the OTA report holds that US industrial reluctance to staff up for high-temperature superconductivity development derives in part from unwillingness of business managers to sacrifice short-term performance with long-term opportunity. Our Company would not be where it is today if earlier managers had adopted such an expedient, short-term outlook. Most of our products are materials which others use – increasingly in high-tech applications. In almost every case, finding and then qualifying for these commercial approaches is an expensive process involving years of work. I can assure you the innovative culture remains in place with us. There is no question but that achieving a world-scale materials business including being the low-cost producer – the true measure of competitiveness – requires this long-range vision. Without such vision, American Industry may lose this race and others to come.

Thank you.

RKQ/smg

SENATOR GLENN'S
QUESTIONS FOR
DR. JOHN ROWELL

Hearing before the Senate Governmental Affairs Committee
U.S. Efforts to Commercialize Superconductivity
June 28, 1988

(1)

In your testimony, you criticized current government policy toward commercializing technologies -- and especially toward commercializing high temperature superconductivity. You even warned against providing increased government funding without changing "the mind set and vision of those who are turning away from high-risk, industrial R&D."

Please comment on how we can achieve such a change. Do you agree with OTA's assessment that we need something along the lines of a federal technology agency in charge of coordinating government support of commercialization efforts?

(2)

You expressed concern at the reallocation of resources -- especially personnel -- from important, though perhaps not glamorous, projects to high temperature superconductivity. You imply that in both the private and public sectors, not enough attention is being paid to setting priorities systematically for the allocation of scarce R&D funds.

Is it your belief that, from the vantage point of national public policy, private companies are not considering the question of R&D priority trade-offs adequately? If so, doesn't this undermine the Administration's assumption that -- given enough freedom -- the private sector alone will make the best possible decisions for the nation as a whole about the allocation of R&D resources?

(3)

In the area of semiconductors, there are examples of industry collaboration (such as MCC, the Microelectronics & Computer Technology Corp.) and a government-industry collaborative effort (Sematech). How feasible or desirable would either of these joint-venture approaches be for high temperature superconductivity? How can government best induce the institution you work for to embark upon these sorts of efforts?

(4)

I understand that over 50 leading Japanese companies have joined a venture called the International Superconductivity Technology Center or "ISTEC". Reportedly, ISTEC will focus its R&D efforts in areas of high temperature superconductivity that are too difficult or too risky for any one company to pursue alone. In the February 17, 1988 edition of the Wall Street Journal, ISTEC ran an advertisement inviting American companies to join as members. Has Bellcore responded to this offer? What are the advantages and disadvantages of participating in this venture?

Bell
Communications
Research

J. M. Rowell
[illegible]

Room NVC 3Z-367
331 Newman Springs Road
Red Bank NJ 07701
Phone (201) 758-3144

August 18, 1988

Senator John Glenn
United States Senate
Chairman, Committee on
Governmental Affairs
Washington, DC 20510-6250

Dear Senator Glenn

Following the June 28th hearing of your Committee on Governmental Affairs on the report entitled "Commercializing High Temperature Superconductivity" that was issued by the Office of Technology Assessment, you sent me four additional questions. My answers to these are enclosed. Those of us working in this field appreciate your interest in this exciting area of science and technology.

Yours sincerely

John M Rowell

J. M. Rowell

DR. JOHN ROWELL'S ANSWERS TO SENATOR GLENN'S WRITTEN QUESTIONS

Following the Hearing before the Senate Governmental Affairs Committee
U. S. Efforts to Commercialize Superconductivity
June 28, 1988

1. The reasons why the U. S. is turning away from high risk industrial R&D are many. It is not even uniformly true that this is the case, for example in biotechnology and medicine the U. S. appears to be much more visionary than in the solid state sciences, which include electronics and optoelectronics. Often the tax policies of the U. S. are quoted as the primary reason for the decline of long term industrial research, but this seems to ignore the fact that we do have pockets of strong industrial research in the U. S. Merck is an example in the drug industry, AT&T and IBM in the computer/communications industry. AT&T has maintained a very strong research base, despite the financial and other difficulties created by divestiture. Meanwhile other companies have cut back their research programs even though they appeared financially healthy. Thus I am forced to conclude that whether or not a company supports long term research is only slightly affected by tax policies, and to a much greater extent by whether the chief executive officer and upper management believes in the value of such research. It is almost a matter of faith, some believe and some do not!

However, the example of government has been and can be a strong influence on the attitudes of industry. In fields of military importance, federal funding has created companies which carry out exciting long term research entirely under federal contracts. This requires no research support from the company itself, and hence no belief in the importance of research to the future technology. Government policies have not encouraged such companies to transfer this very advanced technology to the civilian sector, or to create such industry themselves.

Thus government policies do influence the nature of industrial research. By example and by funding, civilian industrial research could be encouraged just as military research has been in the past. By policy statements, persuasion, encouragement and financial incentives, industry leaders could be convinced that long term research is just as critical for a healthy balance of payments in the future as it is for a strong defense.

Concerning the need for a federal technology agency, I do not believe that the U. S. requires just another funding agency for civilian research. Rather I would prefer to see a new agency addressing anomalies in funding by today's agencies. For example, I read that Paul Chu at the University of Houston is likely to receive $30M per year for his superconductivity research, while the new Science and Technology Centers of the NSF might receive $20M-30M for all research fields. While I agree that Paul Chu should be funded, this imbalance is inexplicable to me. Thus I feel that the role of a federal technology agency should be to examine overall funding as it could affect the civilian sector, and apply funds in selected areas in order to enhance the value of research that is at present being directed only to defense. This agency could also serve the valuable purpose of encouraging more participation by industrial research leaders in the nation's science policy, as is common in Japan.

2. Your question regarding the setting of research/funding priorities has been largely answered in my comments above. As an individual scientist, it is very difficult for me to discern any national science policy, except that to ensure a strong defense based on advanced technology. In fact, I doubt that such a national science policy exists for the U. S. industrial base.

Your question, whether the private sector alone will make the best possible decisions about the allocation of R&D resources, is also an assumption. The assumption is that the U. S. has a balanced and healthy industrial base, across all the important fields, which is then able to make these decisions. This assumption seems to be incorrect, in that the U. S. is no longer competitive in a number of fields, for example transportation (high speed trains), cameras (despite developing sophisticated instruments for exploration from space), microelectronics, optoelectronics and displays. That these industries have declined or disappeared seems to me to answer the question, in that it is clear that U. S. industry in the past has not made the decisions that would have allowed it to survive in the long term, as least in certain technologies. Thus I disagree strongly that the private sector alone will make the right decisions to ensure both the correct use and the correct magnitude of R&D funds.

3. From the point of view of policy, superconductivity should be regarded as two fields. Technology using the conventional low temperature materials is relatively mature and is carried on by small companies. They seem at present to competitive with those abroad. The field of high temperature superconductivity is dominated by very real materials problems that have to be solved by research. Thus I would suggest that, in this case, collaborations should be encouraged at research frontiers, for example between universities, federal laboratories and industrial research.

The interest of my company in such a joint research center would be enhanced if the joint venture was of the highest quality, i.e. if our own scientists saw people in the consortium that they would want to collaborate with. It would clearly be of interest to us if we received research funding for work carried on either at the center or at our laboratories coordinated with the center's activities. There seems to me no reason to assume that funds should flow only to universities and federal laboratories if such a center were created. The transfer of "high T_c technology" to the "low T_c companies," that are at present competing in the market place, should be a major focus of such a center, or centers.

4. We have contacted "ISTEC" to determine the advantages of joining at the center level. It appears that there is nothing to be gained beyond information that is also available in the open literature. One of our scientists has been invited to the first ISTEC conference and we expect to evaluate our position again after that meeting. The second level of participation allows joint research activity in the center in Japan. The advantages are clear, as it will be one of the largest, and probably the best focussed research activity in superconductivity world wide. The disadvantage is the cost, comprising both the fee and the support of research personael in Japan. The price is simply too expensive for me to afford from my current budget, and I do not see prospects of this situation changing.

SENATOR GLENN'S
QUESTIONS FOR
PROFESSOR M.R. BEASLEY

Hearing before the Senate Governmental Affairs Committee
U.S. Efforts to Commercialize Superconductivity
June 28, 1988

(1)

Both you and Dr. Rowell criticized current government policy toward commercializing technologies -- and especially toward commercializing high temperature superconductivity. Dr. Rowell even warned against providing increased government funding without changing "the mind set and vision of those who are turning away from high-risk, industrial R&D."

Do you agree with Dr. Rowell's assessment? If so, how can we achieve such a change? Do you agree with OTA's assessment that we need something along the lines of a federal technology agency in charge of coordinating government support of commercialization efforts?

(2)

In his testimony, Dr. Rowell expressed concern at the reallocation of resources -- especially personnel -- from important, though perhaps not glamorous, projects to high temperature superconductivity. He implied that in both the private and public sectors, not enough attention is being paid to setting priorities systematically for the allocation of scarce R&D funds.

Is it your belief that, from the vantage point of national public policy, private companies are not considering the question of R&D priority trade-offs adequately? If so, does this undermine the Administration's assumption that -- given enough freedom -- the private sector alone will make the best possible decisions for the nation as a whole about the allocation of R&D resources?

(3)

In the area of semiconductors, there are examples of industry collaboration (such as MCC, the Microelectronics & Computer Technology Corp.) and a government-industry collaborative effort (Sematech). How feasible or desirable would either of these joint-venture approaches be for high temperature superconductivity? How can government best induce the University you work for to embark upon these sorts of efforts?

STANFORD UNIVERSITY
STANFORD, CALIFORNIA 94305

DEPARTMENT OF APPLIED PHYSICS

(415)723-1196
Beasley@sierra.stanford.edu

August 17, 1988

Senator John Glenn
Chairman
Committee on Governmental Affairs
United States Senate
Washington, DC 20510-6250

Dear Senator Glenn:

On the basis of my testimony before your Committee on Governmental Affairs in June, you have written me with some specific questions. I am writing now in response to those questions.

1. I think what both Dr. Rowell and I were worried about is that the government would respond with a large crash program, not a longer range more pluralistic approach. The commercialization of superconductivity will not happen quickly, nor is it clear what applications really make sense. We are almost certainly to guess wrong at this point in time, if we were forced to try. It is even likely that we have not yet "invented" the truly revolutionary applications. Also, these are complex materials—which also may be why they are so spectacularly better than the more familiar and simpler conventional superconductors—and will take time to master.

We also were concerned with the continued lack of strong interest broadly in industry in long-term research. Dupont and Bell Core, from whom the committee heard, are not typical. Most companies do not do long-term research as they do. From the perspective of someone in a university, there are not enough people on the other side (i.e., in industry) to receive the knowledge base that we produce. There certainly is in Japan. We all have our favorite stories about how something we learned was used in Japan.

Many industries seem to want "prepackaged technology," [illegible] in the form of intellectual property rights. In my judgement there is too much [illegible] on intellectual property rights and not enough on knowledge base. The exceptions are the large [illegible] companies, who want our graduate students, and the small start up companies [illegible] technical base. These are currently the best forms of technology transfer we have.

The problem I have described above has led to [illegible] the university to go further in developing specific products and to [illegible] direction from industry—to move the line between open research and [illegible] the university. I am one who resists these forces. I do not believe [illegible] optimally. It is pushing universities in an unnatural direction and [illegible] base, which is not to say we do not have a responsibility to transfer technology. [illegible] the line between what should be done in the universities and what should be done in industry. Strong interaction between universities and industry is necessary, but it will work best in my judgement if the proprietary interests are on the industrial side.

You also ask about a federal technology agency. I have not addressed this question deeply in my own mind, but I do believe that we need more forces speaking for commercial technology in the federal balancing of resource allocation. The NSF is sometimes asked to do this, but it must also speak for unfettered basic research. The two roles sometimes conflict. DoD got somewhat of a bum rap in the OTA report. DoD has been a very good supporter of basic scientific and engineering research in the universities. It supported research much more broadly before the Mansfield Amendment of years past. There is no doubt that we are still living with the effects of that amendment.

2. I believe that Dr. Rowell was concerned with the possibility that both government and industry would be content with shifting existing resources to high-temperature superconductivity at the expense of other work. I agree that this would be dangerous and leave other areas of research uncovered. To put it bluntly, I think both he and I would argue that the nation (both government and industry) should put more resources into materials research. It is not a question of allocation of resources between areas of materials research. I know this sounds like the familiar refrain: more, more, more. But I firmly believe that more resources are required in this area. I believe it would have happened if materials research were one form of big science, which our nation seems to favor as a paradigm of science. Materials research is small science in style, only big in aggregate. I believe the nation must become more sophisticated in its understanding of science and technology. We are still living with a view of science that was an outcome of the Manhattan Project. It does not serve us completely well, in my view.

I would judge that private companies can make decisions between R&D priorities. The problem is the time frame they consider in that decision making process and the total amount of their resources they are willing to use. We simply do not think long term enough. I do not believe the current Administration's assumption has been borne out, at least as regards industry's willingness to invest in long-term, commercially-oriented research.

3. This question basically asks, what is the best way to martial the size of effort necessary to take advantage of the opportunity that high-temperature superconductivity respresents. My view is that we need a pluralistic approach. We do need, however, some relatively large centers of excellence with an adequately long-term mandate to solve the materials problems associated with this technology, and to provide the knowledge base for the applications. They might focus on the materials problems associated with electrical (power applications) or electronic applications, but should not focus on a specific product. I believe they should be set up in association with universities, because that is where the best research capability in this area exists currently in our country. These centers must take their technology transfer responsibilities very seriously, but industry must play its part (see my remarks above) if this is to work. There would be institutional experiments in how best to couple the universities and industry. As the materials problems become manageable, many other institutions can begin to take advantage of the technological opportunities that these new superconductors present.

I hope this is responsive to your queries, and I very much appreciate this opportunity to make input to the Congressional processes. It has been an interesting "civics" lesson for me if nothing else.

Sincerely,

M. R. Beasley

M. R. Beasley
Chairman and
Professor of Applied Physics

MRB:jcc

STATEMENT OF
D. BRUCE MERRIFIELD
ASSISTANT SECRETARY OF COMMERCE
FOR PRODUCTIVITY, TECHNOLOGY AND INNOVATION
BEFORE THE SENATE GOVERNMENT AFFAIRS COMMITTEE

June 28, 1988

Mr. Chairman and members of the Committee, thank you for this opportunity to discuss the economic linkages between the development of advanced technologies -- such as those associated with superconductivity -- and U.S. industrial competitiveness.

First, let me emphasize that the U.S. has many advantages over other nations competing in worldwide market places. I realize that this statement flies in the face of some contemporary analyses, most of which tend to impart a negative flavor to our global economic posture. And, we do have a number of challenges to meet in upgrading our capacity to produce quality products at competitive prices. However, we must not overlook several unique factors which cast a most positive light on our competitive capacity. They include:

- a research and development capability that is second to none, with Federal and private sector expenditures exceeding $132 billion in 1987. Compared to other countries, the U.S. still spends in absolute terms more on R&D than the next four largest western industralized countries combined (Japan, Federal Republic of Germany, France, and the United Kingdom). We produce more basic research than any other nation largely because of our excellent university and national laboratory systems.

Federal funding for basic research overall increased 64 percent since 1981, while science research increased 60 percent and engineering research, 100 percent;

- the U.S. has the greatest breadth and depth of technical-industrial capability in the world to translate new discoveries into useful products, processes and services;
- the U.S. has a remarkable entrepreneurial culture which provides permission to fail and try again until success is achieved without permanent personal or public penalty;
- the U.S. has the largest pool of 4.5 million trained scientists and engineers in the world; and,
- the U.S. has the most flexible capital development capability in the world's largest financial market.

Secondly, the current explosion of advanced technology is creating unprecedented industrial opportunities. Examples include:

- the development of advanced materials such as ceramics, super alloys, ultra-fine particle composites, and superconducting materials;
- the discovery and production of synthetic enzymes and single function proteins;
- the ever-advancing conquest of disease and the prolongation of life through the sequencing of the human

genome leading to correction of genetic defects, and the progressive elimination of viral diseases;

- the progress that has been achieved in ever increasing computer sophistication through parallel processing, artificial intelligence, and advanced architecture;
- the futuristic helium 3 mining of the moon, which will eliminate neutron flux and might provide energy at the equivalent cost of $7.00 for a barrel of oil. A dividend of this process may be the virtual elimination of adverse environmental consequences to planet earth;
- the use of pico-second chemistry via laser implants of functional molecules at micro sites; and
- the development, refinement and production of higher definition television (HDTV) receivers, which will enable the U.S. to recapture this important segment of the consumer products industry.

The overriding reality is that it is technology and its effective application to U.S. industrial and service sectors that will continue to be the main lynchpin in U.S. competitiveness and, consequently, U.S. economic growth. The key point is that the United States, with its vast scientific and technological infrastructure and market, has the capability to be the world leader in practically any advanced sector. To do so, however, we must strengthen our primary long run comparative advantage -- knowledge and its application in continuously creative ways.

One of our greatest limitations in this regard is our comparative weakness in transforming research results quickly into high quality products and processes for the U.S. and other global markets. I am distressed that, a decade from now, our marvelous inventions in such areas as superconductors and biotechnology may appear in the form of billions of dollars of foreign imports yet in only millions of dollars for the U.S. in the form of royalty payments for the technology we invented but failed to commercialize ourselves. The VCR is a case in point.

Some of the blame for this weakness must rest on U.S. businesses which have not adequately factored change into their strategic thinking. Many companies see their business environments as static in an increasingly dynamic world. Simply stated, how can a business make the hundreds of tactical decisions required in any given year if it has no clearly articulated vision of where it wants to be in 10 or 20 years. Strategies must reflect a future characterized by: extensive industrial restructuring; skill shortages, especially in the technical and managerial areas; increased formal linkages with companies all over the world; widespread use of flexible automation; and a much more proactive role of State and local government in business development, expansion, and diversification.

In all of this, the government role is to create a climate conducive to accelerated investments in innovation and reskilling of the workforce. For example, stability in monetary and fiscal policy is a must, as is financing for basic research. But we must also provide incentives, remove policy barriers, provide information and, where appropriate, supply catalytic services that individual businessess cannot supply for themselves. The primary focus of these should be in three areas: (1) strengthening the effectiveness and efficiency of the process of technological innovation, (2) promoting greater use of flexible automation in U.S. factories, and (3) increasing the ability of the nation's foremost asset -- its skilled labor force -- to continuously renew itself through training and education.

The nation finds itself now at just the beginning of a dramatically expanding set of technological opportunities. We at the Department of Commerce are doing all we can to assist the private sector to exploit these many new opportunities.

In terms of barrier removal, Commerce played a key role in the process that led to the passage of the National Cooperative Research Act of 1984. This modernized the application of antitrust laws to encourage cooperative R&D projects. To date, there are seventy-eight consortia registered for the protection of the Act, several of which have registered their projects separately bringing the total ventures registered to over one-hundred.

Through the Federal Technology Transfer Act of 1986, we cleared the way for a competitiveness-enhancing partnership between the private sector and our federal laboratories. This Act calls upon agency heads to allow their lab directors to enter into cooperative R&D agreements with the private sector, state and local governments, and universities. It also permits them to convey to business exclusive rights to inventions made by federal scientists, and establishes a royalty-sharing mechanism to reward the Federal inventors. Furthermore, an Office of Federal Technology Management has been established in Commerce to work with the agencies and their laboratories to make the Technology Transfer Act as effective as possible. Already this office has developed model cooperative and licensing agreements.

Turning to incentives, the Department of Commerce has taken the lead in several areas which deserve mention here.

Since 1981 we have promoted the use of R&D limited partnerships (RDLPs) which have funded an estimated $4 billion of R&D in recent years. While tax law changes have made this type of funding more difficult, it does continue today. There are two important features about this type of funding that have caused us to try to find ways to improve the old RDLP prototype. Of particular importance is the fact that RDLPs fund primarily product development -- that stage of the innovation process which is our weakest link. Without a major influx of funding at the product development stage, the innovation process will remain out of balance. Moreover, RDLP funding represents a net addition to the traditional sources of R&D funds -- the Federal Government and retained earnings. In other words, RDLPs tap a new source of funding -- the private investor -- and augment, rather than divert, existing R&D funding.

In addition, the Department of Commerce has been active in facilitating cooperative private sector efforts.

For example, we have promoted the use of new business incubator centers which have emerged as an important factor in the recent burst of entrepreneurial activity occuring in our country. The contribution that incubators offer is exceedingly timely in that they afford _every_ state and local community an opportunity to diversify its economic base and to create many new jobs, as older industries decline. Also, incubation centers function as vehicles for accelerating the commercial application of new technologies. Over 200 centers have formed in the United States to provide the physical, intellectual, and managerial ambiance that stimulates the chances for success in the open market.

On request, we help groups of firms to form R&D consortia in the belief: (1) that the technology developed by them will be useful; (2) that the productivity of U.S. firms' limited R&D budgets can be increased by avoiding redundancy of effort; (3) that these consortia are a first step in the process by which U.S. firms will learn to cooperate with each other; and (4) that the very process by which these consortia organize themselves forces their member firms to develop long-term strategies.

Next, we have been offering strong encouragement to growth in flexible, computer-integrated manufacturing (FCIM) by catalyzing the formation of small company consortia to invest in shared FCIM facilities. With "sharing" such companies are spared the full initial capital outlays for equipment and the associated, often difficult, equipment and software selection process. Also, they can acquire training at their own pace in how the new FCIM technology works, and even more important, learn how to manage the company as a whole which is quite different and more challenging than simply managing FCIM technology alone. Over time, such facilities can even become "teaching factories." A shared "micro" factory also enables a user-company to compare manufacturing costs and the quality of existing products under the old and new systems. It can also test-run a new product before committing resources for internal automation.

In addition to promoting the shared FCIM concept, we are exploring new ways of helping companies justify financial investments in the FCIM process. For example, we have planned a roundtable in cooperation with Arthur Anderson and Company on new accounting principles that enhance an understanding of the economics of FCIM systems.

Also, pursuant to Secretary Verity's direction, we have established an interagency forum, which I chair, to identify opportunities for government-private sector partnerships to encourage FCIM use. This forum also reflects our recognition of the fact that -- in addition to helping U.S. firms upgrade their manufacturing -- FCIM systems can contribute to a strengthened industrial base, reduce costs of the DoD spare parts inventory, provide increased surge capacity in the case of a national emergency, and return offshore production of parts and components to the U.S.

Because a skilled U.S. workforce is crucial to helping our industries remain competitive, Commerce is working with the Departments of Education and Labor to strengthen this nation's human resources. My own office has launched a program to stimulate private sector development and applications of technology based learning such as interactive computer-videodisc systems. Among other objectives, this will provide the basis for continuous, lifelong reskilling of the workforce as rapidly changing technology compresses life cycles for acquired skills. In addition, this will help currently employed workers and small businesses to hone their skills so that we can remain competitive in world markets. In this effort, we are working with such organizations as the National Alliance of Business, the Broadcasting Industry Council, and numerous training and education organizations.

Real competitiveness happens at the marketplace level, keyed to the four major elements of business competitiveness: quality, price, delivery time, and the ability to quickly adjust production facilities to meet new market needs. U.S. business must provide the strategic planning and leadership to knit our innovative process into a smoother working system. Private business must be the driving force to use our unparalleled technology base to seize market opportunities with new products and take a world view regarding the research we must perform and the technologies, product design, factories and products that we must develop to compete internationally in the future.

There are a number of areas in which we will be working with the Hill:

First, the Administration has proposed legislation to create a new Under Secretary of Commerce for Science and Technology to bring together the considerable Commerce Department resources into one unit. This new Science and Technology Administration would elevate the importance of these areas and aid the overall goal of ensuring that U.S. excellence in the laboratory works hand-in-hand with U.S. excellence in the market place.

The House Energy and Commerce Committee and the Science, Space and Technology Committee are reviewing the proposed legislation at this time.

Here in the Senate, the Committee on Commerce, Science and Transportation is preparing to markup this legislation in the near future, I believe.

Secondly, the Department of Commerce continues to support the extension and restructuring of the R&E tax credit. The legislation which Senators John Danforth and Max Baucus recently introduced (S.2484) would allow start-up businesses to use the credit and would extend the credit permanently. We believe that this legislation is important for these reasons, and we support it.

However, the legislation as it now stands is limited in its ability to help new firms that typically have no tax liabilities, i.e. that are not yet taxpayers. For this reason, we are continuing to explore a new or improved financing vehicle which would attract private investor funding for R&D and which would be eligible for existing tax incentives. Such an investment vehicle would have the additional advantage of bolstering the product development end of the innovation system, which, as you well know, is our greatest weakness.

Finally, the President has forwarded to the Congress his omnibus proposal for prompting further discoveries in the field of superconductivity and for ensuring U.S. readiness in rapidly commercializing technologies resulting from recent and anticipated scientific advances. As you know, the bill is entitled the "Superconductivity Competitiveness Act of 1988." The President's legislative proposal is part of a broad initiative to continue American leadership in the rapidly emerging superconductivity field. It has three major components:

- amendments to the National Cooperative Research Act of 1984 ("NCRA") to expand its coverage of joint R&D ventures to include some types of joint production ventures;
- amendments to U.S. patent laws to provide protection for products made by patented processes; and
- authority to permit Federal agencies to withhold from release under the Freedom of Information Act ("FOIA") certain commercially valuable scientific and technical information generated in government-owned, government-operated laboratories.

I believe that amending the NCRA to cover joint production ventures is a particularly important step that would ease the risk of antitrust litigation perceived by U.S. businesses that could otherwise benefit from pro-competitive collective efforts to improve productivity.

Increasing the protection that U.S. patent law affords to U.S. process patentees will eliminate the harm suffered by owners of process patents who discover that someone abroad has pirated their processes to manufacture products for export to the United States. Improving our patent laws in this area will properly reward and thus stimulate innovative efforts by U.S. firms to discover new, more efficient manufacturing processes.

Finally, it is vitally important in improving U.S. competitiveness that U.S. firms and entrepreneurs reap the benefits of commercially valuable scientific and technological data and information developed in United States Government laboratories and similar facilities. Wholesale disclosure of such information under the Freedom of Information Act (FOIA) could seriously harm U.S. economic competitiveness. The President's proposal is designed to avoid such disclosure.

I hope that the Congress will act favorably on this proposed legislation.

Mr. Chairman, as I said at the beginning of this statement, the United States has many advantages over all other nations, not only to recapture and maintain technical-industrial leadership in most areas of commerce, but to provide international leadership in expanding the global economy. I hope that we can work together in providing the proper climate to achieve that end.

Thank you, Mr. Chairman. I will be happy to answer any questions the Committee may have.

RESPONSES OF MR. MERRIFIELD TO WRITTEN QUESTIONS SUBMITTED BY SENATOR GLENN

Q.1. The federal government spends more on high temperature superconductivity research and development than any other nation. Yet, Dr. Graham states in his testimony that "the Japanese government has a superconductivity research initiative under way, which ... appears to have lower funding than our own, (and yet is) ... formidable and well-focused." Are we paying more money for less results? Why isn't it *our* more expensive program that is formidable and well-focused?

A.1. The U.S. effort is fragmented among many independently working universities, government labs and a *few* companies... doing mostly federally funded *basic* research to elucidate the mechanisms and the theory.

The Japanese, however, are systematically trying all the combinations of all the appropriate elements in the periodic table -- to see if anything works. This is how they found the *Bismuth* formula. They don't do redundant work as we do -- each lab has its own series to run through.

Moreover, U.S. industry traditionally has been hampered in the *application* of new technologies by the relatively high cost of U.S. capital (about 3 times higher here than in Japan). In fact, if it were possible to make a true accounting of the hidden subsidies affecting the cost of Japanese capital available for industrial application work, it probably would be close to zero. This means that Japanese industry has almost zero long term risk associated with such developments. To stimulate U.S. private sector investments of this type, the cost availability of risk capital must be addressed.

Finally, for many years the Federal Government has lacked a coherent set of principles for encouraging federal-private sector collaboration and for commercializing the technology produced at federal laboratories, or by contractors operating under federal funding arrangements. Since 1980, the Bayh-Dole Act, the 1984 Amendments thereto, the Federal Technology Transfer Act of 1986, and President Reagan's Executive Order 12591 have all addressed this need and have set in motion a series of initiatives that should lead to more successful research programs. This is because these measures recognize the basic truth that those who are closest to technology understand it best, and -- with proper incentives -- will take the steps necessary for achieving its commercial potential.

The establishment of the Office of Federal Technology Transfer reflects the importance that the Department of Commerce attaches to this important tenet too.

Q.2. In Dr. Graham's criticism of OTA's recommendation to set up a Civilian Technology Agency, he argues that the mission orientation of DoD and DoE is "vital to our national security and must be compromised by centralizing funding of specific areas of research in one agency."

Is it possible that effective commercialization of new technologies is also vital -- vital to our nation's economic security? Does the existence of agencies with different missions preclude the establishment of a new agency with a mandate to promote commercialization of technology?

A.2. These two positions need not be incompatible, and Administration-supported legislation has been introduced to create an Under Secretary of Commerce for Technology that would reconcile both. That office would be responsible for coordinating fragmented efforts in order to avoided redundancy and to distill out the "greater wisdom" that usually is inherent in an independently creative society. The Commerce Offices of Productivity, Technology, and Innovation and Federal Technology Transfer would be placed under that Under Secretary.

Already, Commerce (particularly the Office of Productivity, Technology, and Innovation and the Office of Federal Technology Transfer) has pursued this role as follows:

o DoC has initiated and worked with the Congress to enact the National Cooperative Research Act of 1984 which has resulted in the registration with Justice and FTC of over 100 R&D consortia involving over 1000 companies;
o DoC has championed major changes in the law to encourage technology transfer from Federal laboratories (i.e. laws of 1980, 1984, 1986). The U.S. biotechnology industry can be said to have sprung from the 1980 law, and we expect comparable results from the 1984 and 1986 laws and related activities;
o DoC has initiated an effort to encourage industry adoption of flexible, computer integrated manufacturing (FCIM) systems, including the formation of an interagency support committee (which includes the DoD). One aspect of this is a public awareness campaign to promote the concept of shared FCIM facilities among groups of both small second and third tier companies, and large companies such as those in the Sematech model as well;
o DoC is championing the development and application of advanced education technology, especially interactive computer-videodisc systems, which may literally revolutionize education and training in our schools, factories, offices, and homes;
o DoC, through its INPACT program, has created a means by which smaller U.S. companies can find joint venture partners in other countries in order to access foreign technology and expand their markets;
o DoC provides assistance to U.S. companies in metric conversion, so that they can more easily export products to

foreign markets; and,

- DoC is championing other proposals, such as changes in the antitrust laws to encourage joint manufacturing ventures, and in the tax laws to extend and modify the R&E tax credit.

Building on the foregoing record of achievement, this new functional entity would promote and coordinate policy changes and programs which are crucial to the commercialization of new technology without disturbing the on-going missions of other federal agencies such as Defense.

Finally, unlike the CTA situation which Dr. Graham warned against, the mission of agencies such as DoD and DoE would not be compromised by the Commerce reorganization. Should that reorganization materialize, Commerce would continue to initiate and advocate policies for transferring federal technology as that concept is reflected in the Federal Technology Transfer Act. The objective of that Act is not to induce agencies to shift their focus away from research important to their missions and programs but, rather, to give them an incentive to perform that research with an eye toward exploiting its commercial potential. The Act builds on what they do in support of their programs; it does not try to shift the focus of their research.

Q.3. How well equipped are the national laboratories to achieve the government-industry collaboration in R&D that is so necessary for effective technology transfer? Would approaches other than those used now be more fruitful? How might a Civilian Technology Agency, such as that described by OTA, help promote government-university-industry cooperation?

A.3. Government lab personnel are more like those in universities than in industrial laboratories ... focused mostly on basic or very early-stage discoveries and often not fully cognizant of the multiple commercial possibilities that are inherent in their work. What is needed in each laboratory is a small group of industrially trained people who (1) can search out technology that has commercial potential, (2) see that it gets effectively patented, (3) build an incipient business plan, and (4) see that it gets appropriately licensed.

Also, this group would be responsible for putting together consortia of companies to jointly invest in government lab programs that might serve both the labs' mission and commercial interests.

The Commerce Department has been assisting a number of laboratories to do this in a limited way, through an intern program that provides (1) the necessary skills, (2) industrial consultants, and (3) a computerized database of 100,000 U.S. companies that might participate. Also, DoC has established the National Technical Information Service (NTIS) as a "one-stop world-scan" data base that inventories about 85% of world technology, and provides the search capability that is needed in assessing the commercial potential of each opportunity.

Finally, DoC has provided technology assessments for critical areas of advancing technology as a strategic base for developing realistic business plans. What is missing is seed funding for the proposed Commerce Under Secretary to continually update (1) competitive assessments, (2) convene conferences to discuss the findings of those assessments, and (3) provide non-interventionist, technical training and assistance in forming business plans and consortia to develop and commercialize federal laboratory technology quickly.

Also, tax incentives are needed urgently for both research and development limited partnerships (RDLPs) and for venture capital. These private sector funding sources are uniquely market-driven and are complementary to each other (RDLPs' for the pre-commercialization stage, and venture capital for start-ups and subsequent early growth stages).

Q.4. In his testimony, Dr. Gibbons calls high temperature superconductivity, "one of those rare opportunities appearing every decade or so." His description highlights both the importance and the uniqueness of the rapid, exciting discoveries in the high temperature superconductivity field. Do these characteristics make high temperature superconductivity a poor test case for a federal R&D program devoted to commercializing civilian technologies? Does high temperature superconductivity raise the same problems and opportunities as less publicized -- and perhaps more mundane -- commercialization projects?

A.4. Federal involvement in commercializing technology is justified only in "big-technology" areas which are beyond the risk-tolerance of individual companies or of consortia of companies to do alone. For example, Sematech involves 13 large companies which are collaborating to develop a 16 megabit flexible chip manufacturing facility. Not even IBM or AT&T can do this alone. Even 13 companies in cooperation have required 40% government funding to mount a $1 billion 4 year program. The alternative is to lose the "mass memory" segment of this business to foreign nation targeting. Loss of this segment would have both industrial competitiveness and national security consequences.

The Sematech model also will apply in varying degrees to segments of superconductivity, high definition television (HDTV), biotechnology, automation of the housing industry, space commercialization, flexible computer integrated manufacturing, advanced ceramics and other emerging technologies. Each situation is its own special case, with the government role being one of: (1) seed funding a private sector managed competitive assessment of the opportunity; (2) catalyzing the formation of a private sector consortium to develop a business plan that incorporates both private and public capabilities as needed; and (3) providing funds for the private sector managed and directed program only to the degree needed. Superconductivity is probably no more revolutionary then biotech, FCIM, or the computer and is therefore probably a fair test case. The advantages and limitations of Federal funding in any technology will be the same. But once economically feasible superconductivity materials are discovered, then the private sector will have to do what it alone can do. Consortia of utilities can install new grids and electric energy storage systems (perhaps requiring some initial federal assistance to demonstrate the economics involved), while individual companies can produce new electric motors, transistors, and products involving hundreds of other creative ideas -- but without federal assistance.

Q.5. Dr. Graham testified that the private sector is responding to the President's Superconductivity Initiative. Does this mean the Administration disagrees with the OTA conclusion that U.S. corporations are falling behind in efforts to commercialize high temperature superconductivity technologies? Do you agree with Dr. Rowell's statement that without IBM and AT&T, "U.S. industrial research would be in deep trouble?"

A.5. Only a few U.S. companies are doing much application work on new-composition of materials exploration such as the Japanese are consistently doing. Because technology often drives basic research (discoveries often occur accidently, but are only later explained scientifically) we may need a more balanced focus not only on basic research but also on specific applications. These two approaches are, by nature, synergistic.

PROPOSED TESTIMONY
OF
DR. WILLIAM R. GRAHAM
SCIENCE ADVISER TO THE PRESIDENT, AND
DIRECTOR, OFFICE OF SCIENCE AND TECHNOLOGY POLICY
BEFORE THE
SENATE COMMITTEE ON GOVERNMENTAL AFFAIRS

Room 342, Dirksen Senate Office Building
June 28, 1988
9:30 a.m.

FEDERAL GOVERNMENT'S ROLE IN ENHANCING SUPERCONDUCTIVITY TECHNOLOGY DEVELOPMENT AND APPLICATION IN THE U.S., AND HOW SUCH EFFORTS COMPARE WITH THOSE OF OUR MAJOR TRADING RIVALS

Mr. Chairman:

The topic of today's hearing is of great importance to this nation. Superconductivity is a phenomenon which, when fully developed and harnessed for the good of humankind, will remold the traditional patterns of products in the marketplace. We can envision the ubiquitous personal computer endowed with supercomputer capabilities, or hand-held calculators turned into personal computing engines. Physicians will be able to diagnose disease in their offices by seeing computer-enhanced images of suspect organs. The energy and natural resources consumption of our civilization will be sharply reduced by newly efficient methods of generating, transmitting, and using power, and by new methods of separating and processing the raw materials from which consumables are formed. Finally, we all look forward to an array of new products that we cannot yet envision.

A technology with the potential to change the commercial world so profoundly as superconductivity cannot be ignored. Companies that expect to prosper in the 21st century must set a course today to ensure their access to the technology. In the broader sense, nations that expect to prosper must set a course to ensure the incorporation of this critical technology into their industries.

The President recognized the importance of the basic discovery of superconducting perovskites in early 1987, and chose a course of action to ensure this nation's leadership in the future technologies. We are now moving forward aggressively to enhance superconductivity technology development and commercialization, and I welcome the opportunity to present my perspective of the progress.

History of the Federal Role in Superconductivity

Superconductivity was first discovered in 1911. Even with the constraints of operating at liquid helium temperatures, superconductivity offers some unique capabilities, much appreciated by the scientific community. The federal government has been supporting research on superconductivity since the 1940s.

This sustained federal commitment to basic research made possible the current niobium-based superconductor technology, which is the mainstay of today's magnetic resonance imaging systems, superconducting particle accelerators, and even the electromagnetic energy storage systems in the design stage.

This Administration remains committed to supporting low-temperature superconductivity research. Federal spending in FY 1987 reached nearly $40 million, and should increase to a requested $70 million in FY 1989. Most of these funds reside in agencies whose missions include non-military functions, such as the National Science Foundation, National Aeronautics and Space Administration, and the Department of Energy.

Even in the Department of Defense, the majority of funding for low-temperature superconductivity in FY 1988 and FY 1989 will be dedicated to electromagnetic energy storage systems--a dual use technology that stands to provide extensive spin-off benefits to the commercial sector.

Support for low-temperature superconductivity provides an essential foundation for later application of high-temperature materials.

The discovery of superconductivity at liquid nitrogen temperatures in January 1987, had a great impact on the scientific world. The significance of that breakthrough went far beyond raising the critical temperature. The profound effect was the shattering of theoretical understanding of the phenomenon itself, along with its temperature limits. If we could now achieve superconductivity at liquid nitrogen temperatures, albeit with brittle ceramics, could we not also achieve superconductivity at room temperatures?

President Reagan was quick to respond to the long-term significance of these research breakthroughs and developments. In the spring of 1987, he quickly mobilized the executive branch to respond both within the government apparatus and the larger private sector. The broad, future-oriented vision of the Reagan Administration was highlighted in the Federal Conference on Commercial Applications of Superconductivity, held in July 1987. The President's Superconductivity Initiative was issued at the same time, specifying a number of concrete actions.

The Conference was an assembly of 1,400 individuals from government, industry, and universities, who have a direct interest in American competitiveness. Its purpose was aptly described by President Reagan in his address when he said "we are here at this conference, so that business and science can cross-fertilize, can begin at these early stages to dream and plan together -- because this new age of superconductivity is a new arena for the spirit of enterprise." The Conference alerted our nation's scientists, engineers, and entrepreneurs to the opportunity at hand, and to the commitment of the Administration to encourage exploitation of superconductivity. The private sector was reminded that they had the ultimate responsibility to translate the basic research discoveries of this exciting new field into commercially viable products. This challenge from the President is often repeated in speeches by Cabinet officers and prominent leaders of the science and technology community today.

The U.S. private sector is responding, not only in the established laboratories of major companies, but with new ventures. Entrepreneurs are planting the seeds that are sprouting into an innovative superconductivity industry. Venture capitalists, believing that this industry will flourish, are investing in startup companies, with as much as $5 million in first-round financing.

The President's Superconductivity Initiative

The commitment of the Administration was expressed in the President's Superconductivity Initiative, which defined Federal Government actions across a broad front. Some of the key actions included:

- The Superconducting Materials Subcommittee (SMS) of the Federal Coordinating Council for Science, Engineering and Technology Committee on Materials (COMAT) was established, having its first meeting in May 1987. Its purpose is to coordinate superconducting materials research conducted by the agencies of the federal government, to identify relevant issues and concerns, and to bring them to the attention of COMAT. Over the course of its six meetings to date, representatives on the SMS have exchanged information on agency programs, plans, and studies. Through SMS coordination the DOE Ames Newsletter, High Tc Update, was expanded to provide coverage to more than 2,000 subscribers throughout the world. The SMS is also completing a description of federal research programs in superconductivity.

- Current flexibility in R&D funding permits a redirection of funds to targets of opportunity or breakthroughs of national interest. The federal system responded to the discovery of high-temperature superconductivity by redirecting funds to get programs underway in that (1987) fiscal year. In total, $44.9 million was directed to high-temperature superconductivity. With guidance from the President's Superconductivity Initiative, the total agency spending for high- temperature superconductivity research in FY 1988 is estimated to be $94.8 million, and is projected to rise to $135 million in FY 1989. In total, the U.S. government has committed more funding to superconductivity research than has any other nation. And this has been accomplished within current federal budget constraints.

- An early product of the Initiative was the designation of certain superconductivity centers within the federal agencies and their laboratories. These centers are in place and actively developing the enabling technologies necessary to commercialize superconductivity.

- As a result of the Initiative, a Superconductivity Advisory Group has been established to advise the Administration on research and commercialization policies related to superconductivity. Chairman of the group is Dr. Ralph E. Gomory, IBM Senior Vice President for Science and Technology and a member of the White House Science Council. The Executive Secretary is Dr. Praveen Chaudhari of IBM. Members are Dr. Kent Bowen of MIT, Dr. John Foster of TRW Inc., Dr. Theodore Geballe of Stanford University, Mr. Kenneth Oshman, a private investor, and Dr. Robert Schrieffer of the University of California, Santa Barbara. The group has met several times and will report their findings periodically.

- The legislative changes identified in the Initiative have been incorporated in the Superconductivity Competitiveness Act of 1988, proposed to Congress in February 1988. Briefly, the Act amends the National Cooperative Research Act to cover joint production ventures, increases the protection of the U.S. patent laws for holders of U.S. process patents, and provides protection for certain commercially valuable scientific and technical information generated in government-owned and -operated laboratories. This last provision relieves government scientists of the compulsory obligations to disclose commercially valuable scientific data under Freedom of Information Act requests.

- Among the other actions from agencies of the government, the Department of Defense Superconductivity R&D Options, published in July 1987, has produced a "map of territory worth exploring" to serve as a menu for a five-year DOD program in high-temperature superconductivity. Most of the DOD applications--e.g., sensors, Josephson-junction electronics, magnets, rotating machinery--are dual-use technologies that will serve the U.S. commercial sector, as well as our national security interests.

Superconductivity Programs in Other Nations

Focusing now on the international aspects, all signs point to intense competition.

The Japanese government has a superconductivity research initiative under way, which by official Japanese government figures appears to have lower funding than our own, but is still formidable and well focused. Although we don't have accurate figures to quantify Japanese private sector investment, it is clear that many of their large corporations have targeted the development of superconductivity technology. Add to this Japan's orientation toward commercialization, and a logical conclusion is that the U.S. private sector must prepare for a long and protracted fight for market share.

The competition from our other trading partners, while not so concentrated, is diverse and significant in its totality. The Federal Republic of Germany has 70 research groups in universities, federal laboratories, and industry at work on superconductivity. France and the United Kingdom are each developing broad-based research programs. India, China, and Australia also have viable programs under way.

Although the international competition, particularly from Japan, will be intense in the coming years, the Administration has defined an overall policy for science and technology interactions based on balance and reciprocity with other countries. This policy essentially "levels the playing field" for our nation in the science underlying superconductivity. Our tangible goal is to achieve equity in the contributions, risks, and benefits of collaboration. These same fundamental themes are embodied in the General Framework of Principles for International Co-operation in Science and Technology that I proposed at the October 1987 Science Minister's meeting of the Organization for Economic Cooperation and Development (OECD), and that organization was formally adopted on April 21, 1988.

The industrialized countries rightly regard adequate investment and commitment to excellence in the basic sciences as a means of preparing their economies for the challenges of the future. Adequate protection of our inventors' creativity will provide incentives for new and improved products and processes.

In science and technology, as in economic relations, reciprocity and balanced access provide a solid foundation for stable, lasting cooperation. An equal partnership should emphasize those areas of cooperation that promise to benefit both partners equally. Both parties also have a responsibility to provide reciprocal incentives for technology development.

Such incentives include protection of intellectual property rights and other policies designed to encourage the transfer of scientific and technical knowledge to enterprises.

President Reagan, in his Executive Order 12591 of April 10, 1987, directed federal agencies, when contemplating cooperative activities with foreign countries, to take account of the degree of access those countries afford to U.S. entities for R&D and licensing agreements with their counterparts. Such opportunities speed the development of commercial applications from the new knowledge derived from research collaboration.

We have recently concluded a new and precedent-setting agreement with Japan. The U.S.-Japan Agreement on Cooperation in Research and Development in Science and Technology, which the President signed in Toronto on June 20, 1988, incorporates the key tenets of the Administration's international science and technology policy. The new agreement:

- Sets forth broad principles under which the governments of the United States and Japan will conduct their future science and technology relationship;
- Establishes cooperation in science and technology areas of national importance in which both countries have complementary capabilities and from which both countries will obtain equitable benefits (superconductivity is among these areas of research);
- Calls for both governments to provide comparable access to major government-sponsored or supported research facilities and activities, as well as to scientific and technical literature;
- Creates a broad management structure that will oversee the overall science and technology relationship and generate new initiatives and policy recommendations for the two governments to implement to strengthen that relationship;
- Sets forth provisions for the adequate protection of intellectual property and the distribution of intellectual property rights arising from the collaborative activities under the agreement; and
- Details the shared security obligations of the United States and Japan in the area of science and technology information and equipment and states that both countries will support the widest possible dissemination of information, subject to export control and classification procedures.

In the months ahead, we will devote attention to implementation of the new Science and Technology Agreement. One aspect of that implementation will utilize the FCCSET structure, in particular the Committee on Materials (COMAT), to develop potential bilateral activities on superconductivity with Japan. We will also identify opportunities for U.S. researchers to participate in research at Japanese facilities that are government sponsored or supported. Given the President's personal commitment and contribution to this important activity, we expect to convene the inaugural meeting of the High-Level Committee this fall to breathe life into the new agreement and lay the groundwork for our successors.

Thus, from my perspective, there is substantial progress in the U.S. public and private sectors toward the development of superconductivity technology and its commercialization. Recognizing, however, that the Office of Technology Assessment has expended considerable effort to study the current structure of the U.S. response and has proposed some alternatives, I would like to comment on their results.

Comments on the OTA Report

The Office of Technology Assessment should be commended for the breadth and thoroughness of the study reflected in the report, Strategies for Commercialization of High-Temperature Superconductivity. The report is perceptive in recognizing the need for diversity and continuity in federal programs to promote commercialization of superconductivity.

I concur with options favoring increased funding of the National Science Foundation, and believe these proposals are in alignment with the recommendations that this Administration has argued frequently. Research funding through the NSF goes primarily to universities and non-profit organizations, including those that are developing complementary relationships with industry. The awards of NSF grants are substantially guided by peer review, ensuring that the private sector participates in the selection. Characteristically, NSF research is funded on a multi-year basis, thus NSF programs provide funding continuity as recommended by the OTA report.

I also support OTA recommendations to enhance the educational system of our country by broadly supporting the channeling of research into the universities, S&T and Engineering Research Centers programs, postdoctoral fellowships, and upgrading of laboratory equipment -- all in proportion to the scientific merit of the work.

The proposals to strengthen industry incentives through tax credits for R&D are also worthwhile. Extending the R&E Tax Credit, due to expire at the end of this year, is an opportunity to act in this area. Authority and encouragement for personnel exchanges between industries and the federal laboratories is welcome support to the President's Executive Order 12591, "Facilitating Access to Science and Technology."

It is unfortunate that the 1988 budget process did not provide support to the NSF at the level requested by the Administration. Failure to provide the recommended 17 percent increase delayed NSF initiatives to establish the S&T Centers program, which was expected to yield one or more university-based centers focused on superconductivity.

However, among the proposed options listed by OTA, there are also those with which I must differ.

Option 1-1, suggesting that Congress pass single bill specific, multi-year authorizations for each agency involved in high-temperature superconductivity R&D, would be counterproductive. Congressional micromanagement of the federal R&D system would destroy the flexibility in the system that is crucial for responsiveness to the rapid changes in the science.

Likewise, certain provisions under Option 2-9 present pitfalls, particularly those that return money to several State governments for pilot projects, or that favor federal support for R&D centers that have the stongest state support. Such actions by the Congress are likely to take on a "pork-barrel" appearance.

Of the three strategies for federal response proposed in the report, I believe that the administration's existing policy - calling for a complementary relationship between industries, universities, and governments at all levels, including the federal - accomplishes the productive aspects of Option 2 without unnecessarily intruding government into the critical economic decisions of the private sector; an area where government has neither the appropriate incentives nor the appropriate experience.

I am concerned particularly with strategy number three -- the Federal Technology Agency. Support of long-term, high-risk basic research that is beyond the focus and the resources of private industry is an appropriate role for the federal government. However, a Federal Technology Agency to centrally administer the programs would create a bureaucracy that would compromise the diversity and responsiveness that characterize our present system. The mission orientation of agencies such as DOD and DOE are vital to our national security, and must not be compromised by centralizing funding of specific areas of research in one agency. There is also too much risk that a Federal Technology Agency would try to "pick winners," in the end, displacing or misdirecting the private sector in appropriate R&D areas.

A number of these and other proposals in the OTA report have the potential for broad and complex repercussions in the scientific community. I wish my friend Dr. Gibbons of OTA had talked with me or other members of the OSTP as the OTA report was being prepared. In any case, as the Congress begins to consider these proposals, I offer to work with you in assessing their potential impact.

Thank you Mr. Chairman.

Response
of
Dr. William R. Graham
Science Adviser to the President, and
Director, Office of Science and Technology Policy
to
Questions from Senator John Glenn

Hearing before the Senate Governmental Affairs Committee
U.S. Efforts to Commercialize Superconductivity
June 28, 1988

Question: The Federal government spends more on high temperature superconductivity research and development than any other nation. Yet, you state in your testimony that "the Japanese government has a superconductivity research initiative underway, which ... appears to have lower funding than our own, (and yet is) ... formidable and well-focused." Are we paying more money for less results? Why isn't it our more expensive program that is formidable and well-focused?

Answer: The U.S. research programs on superconductivity are formidable and well-focused. It is not that the U.S. is getting less for its money, but that the Japanese programs are larger than they would appear from the public disclosure of funding. Although the U.S. government funding for superconductivity in FY 1988 is estimated at $147.4 million, compared to about $70 million by the government of Japan, we must keep in mind that Japanese budget figures do not include salaries of the researchers, and the relative sizes of budgets are directly affected by the dollar-yen exchange rate. The intent of my statement quoted above was to ensure that we do not underestimate the efforts of the Japanese in superconductivity.

Question: In your criticism of OTA's recommendation to set up a Civilian Technology Agency, you argue that the mission orientation of DOD and DOE is "vital to our national security and must not be compromised by centralizing funding of specific areas of research in one agency."

Isn't it possible that effective commercialization of new technologies is also vital -- vital to our nation's economic security? Does the existence of agencies with different missions preclude the establishment of a new agency with a mandate to promote commercialization of technology?

Answer: There is no doubt that commercialization of new technologies is vital to our country, but commercialization must come through the actions of U.S. industry. Government agencies should not interfere with free enterprise by funding activities that would displace the private sector. The proper role for government is to create a favorable climate of laws and policies in which business can thrive.

Mechanisms are in place for this purpose in existing agencies, such as the Department of Commerce (DOC), Small Business Administration, and others. The Office of Productivity, Technology and Innovation in Department of Commerce aggressively tries to influence policy within the government and encourage business developments outside the government which will result in new products, processes, and plants in U.S. industry.

The National Institute of Standards and Technology (formerly, National Bureau of Standards) and other government laboratories participate in consortia with the private sector for the purpose of developing new technologies of common interest. The singular mission of the Small Business Administration is to aid, counsel, assist, and protect the interests of small businesses, which are the major sources of innovative new products and jobs within this country.

A Civilian Technology Agency, as proposed by the OTA, would duplicate the above listed functions and is therefore not necessary. Such an agency carries with it the legitimate concern that public funds used to develop technologies for business purposes may be too easily misused.

Question: How well equipped are the national laboratories to achieve the government-industry collaboration in R&D that is so necessary for effective technology transfer? Would approaches other than those used now be more fruitful? How might a Civilian Technology Agency, such as that described by OTA, help promote government-university-industry cooperation?

Answer: The national laboratories are very well equipped in terms of personnel and facilities for collaborating with industry. One of their key strengths is the ability to supply the intellectual skills and expensive equipment needed by industry for development of new technology. The historic weakness has been the lack of interest in commercial application of the products of the research from the laboratories. This attitude is now undergoing change as a result of legislation that has been enacted in the past eight years -- e.g., the Stevenson-Wydler Act and the Technology Transfer Act. Giving control of intellectual property to the laboratory operators and scientists is a very effective means to increase their awareness and interest in commercial application of their discoveries.

A Civilian Technology Agency would not have any unique methods for stimulating government-university-industry collaboration that is not already available today among the diverse agencies and organizations of the government. Centralizing this function within one agency most certainly would constrain the patterns of opportunity to fewer options than available within the existing system.

Question: In his testimony, Dr. Gibbons calls high temperature superconductivity "one of those rare opportunities appearing every decade or so." His description highlights both the importance and the uniqueness of the rapid, exciting discoveries in the high temperature superconductivity field. Do these characteristics make high temperature superconductivity a poor test case for a federal R&D program devoted to commercializing civilian technologies? Does high temperature superconductivity raise the same problems and opportunities as less publicized -- and perhaps more mundane -- commercialization projects?

Answer: On the contrary, the excitement produced by the discoveries in superconductivity has added to the ease with which federal R&D programs in the technology have been established. The announcements in the scientific press attracted the interest of many top scientists and there has been little delay in establishing federal programs. Without this interest, it is doubtful that one of our major early responses, the National Science Foundation's "quick start" grant program, would have occurred.

High-temperature superconductivity does not share exactly the same problems as other commercialization projects. Clearly, the interest in the technology has been beneficial, making it easy to attract some of our best scientists to work in the field.

Question: You testified that the private sector is responding to the President's Superconductivity Initiative. Does this mean the Administration disagrees with the OTA conclusion that U.S. corporations are falling behind in efforts to commercialize high temperature superconductivity technologies? Do you also disagree with Dr. Rowell's statement that without IBM and AT&T, "U.S. industrial research would be in deep trouble?"

Answer: It is too early to know whether U.S. corporations are falling behind in efforts to commercialize high temperature superconductivity technology. About the only products of this nature in the marketplace are superconductivity demonstration kits for use in our schools, and both U.S. and Japanese corporations are offering those products. I think we should take heart that there are innovative and aggressive entrepreneurs in the United States, such as Monolithic Superconductors, Conductus, Ceramics Process Systems, and American Superconductors who are pursuing the market. We need to encourage these companies, and government agencies like DARPA are doing this by sponsoring research in promising areas.

There is no doubt that the industrial giants in our country like IBM and AT&T provide a base of industrial research that is essential to our competitiveness in the world. Additional large companies such as G.E., DuPont, and Westinghouse are also providing funding for high temperature superconductivity research. At the same time, we owe a great deal to the innovative, small start-up firms. For example, Apple Computer started just a decade ago with two inventors in a garage, and to this day the Japanese have not been able to break into their segment of the computer market. So, we must appreciate the importance of the small as well as the large companies to our industrial vitality, and expect that our ability to capitalize on superconductivity will come from both types of concerns. In addition, joint ventures with our federal laboratories are occurring, such as Argonne with W.R. Grace, and Argonne also with the Allied-Signal Corporation. DOE has also set up 3 superconductor pilot centers to enhance transfer of government supported work to U.S. industry.

COMMERCIALIZING

HIGH-TEMPERATURE SUPERCONDUCTIVITY

CONGRESS OF THE UNITED STATES OFFICE OF TECHNOLOGY ASSESSMENT

Foreword

Less than two years ago, superconductivity—total loss of resistance to electricity—could be achieved only at temperatures near absolute zero. Since the discovery of high-temperature superconductivity (HTS), research laboratories around the world have pushed the temperature limits steadily upward, opening the way to commercial applications with potentially revolutionary impacts. The scientific race is becoming a commercial race, one featuring U.S. and Japanese companies, and one that the United States could lose. Indeed, American firms may already be falling behind in commercializing the technology of superconductivity.

Japanese companies have been more aggressive in examining possible applications of HTS, and what it might mean for competitive strategy. While payoffs on R&D may lie a decade or more in the future, managers in Japan have been willing to take the risks. Although a number of U.S. companies have also begun major efforts in HTS, most American managers, under pressure to show short-term profits, have been more inclined to wait and see.

So far, the U.S. Government has supported the development of HTS in its traditional way—by putting money into R&D, mostly through the mission agencies. Federal agencies moved quickly to channel money to HTS when news of the discoveries broke. The breadth and depth of the response in government agencies and Federal laboratories, and in the university system, shows the continuing vitality of the scientific enterprise in the United States. Although Federal dollars will help support a technology base that the private sector can build upon, the U.S. Government is not providing direct support for commercialization. Nor have we any policy or tradition for this kind of support—unlike countries such as Japan.

Postwar U.S. technology policy coupled R&D funding with indirect measures, such as tax policy, to stimulate commercial innovation. So long as American companies remained well ahead of the rest of the world in technical skills and management ability, this approach proved successful. With the continuing decline in competitiveness across many sectors of the U.S. economy, it no longer seems good enough.

The Senate Committees on Governmental Affairs, Energy and Natural Resources, and Commerce, Science, and Transportation, together with the House Committee on Science, Space, and Technology requested the assessment of which this report is part. OTA's Energy and Materials Program is also conducting a more comprehensive examination of the science and technology of high-temperature superconductivity, and the future research agenda, as the second part of this assessment. Their report will appear in 1989.

OTA is grateful for the assistance provided by many people inside and outside of government during the preparation of this report. Full responsibility for the contents rests with OTA.

John H Gibbons

JOHN H. GIBBONS
Director

High-Temperature Superconductivity Review Panel

Claude Barfield
American Enterprise Institute

Arden Bement
TRW, Inc.

Harvey Brooks
Harvard University

Kent E. Calder
Princeton University

Jeffrey Frey
University of Maryland

Jacques S. Gansler
The Analytical Science Corp.

Donald A. Hicks
University of Texas at Dallas

David A. Hounshell
Harvard Business School and
University of Delaware

Clifford K. Jones
Westinghouse Electric Corp.

Henry Kressel
E.M. Warburg, Pincus & Co., Inc.

Harry Kroger
Microelectronics & Computer
Technology Corp.

Ira Magaziner
Telesis

William D. Manly
Consultant

Edward Mead
E. I. du Pont de Nemours & Co.

Egils Milbergs
Institute for Illinois

Thomas A. Pugel
New York University

Maxine Savitz
The Garrett Corp.

David J. Teece
University of California, Berkeley

James Wong
Supercon, Inc.

Donald L. Wood
General Electric Co.

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OTA Project Staff: Commercializing High-Temperature Superconductivity

Lionel S. Johns, *Assistant Director*
OTA Energy, Materials, and International Security Division

Audrey Buyrn
Industry, Technology, and Employment Program Manager

John A. Alic, *Project Director*

W. Wendell Fletcher, *Deputy Project Director*

Dorothy Robyn

Martha Caldwell Harris

Clare Delmar

Jane A. Alexander

Carollyne Hutter

Contributors

Gregory Eyring

Laurie Evans Gavrin

Contractors

Joan Lisa Bromberg
The Laser History Project

Kenneth Flamm

Glenn R. Fong

Robert R. Miller

David C. Mowery

David Sheridan

Granville J. Smith, II

W. Henry Lambright, Maureen O'Neill Fellows, and Leslie A. Jones
Syracuse University

Administrative Staff

Edna M. Thompson, *Administrative Assistant*

Diane D. White, *Administrative Secretary*

Workshop on High-Temperature R&D in Japan and the United States, January 1988

Ronald H. Baney
Dow Corning

Fernand D. Bedard
U.S. Department of Defense

Justin L. Bloom
Technology International, Inc.

Roger W. Boom
University of Wisconsin

H. Kent Bowen
Massachusetts Institute of Technology

Richard C. Bradt
University of Washington

Donald W. Murphy
AT&T Bell Laboratories

Jeffrey Frey
University of Maryland

William Gallagher
IBM

Richard Harris
National Bureau of Standards

Young B. Kim
University of Southern California

M. Brian Maple
University of California, San Diego

Edward Mead
E.I. du Pont de Nemours & Co.

Charles T. Owens
National Science Foundation

Robert Pohanka
Office of Naval Research

Richard J. Samuels
Massachusetts Institute of Technology

Roland W. Schmitt
Rensselaer Polytechnic Institute

Joseph L. Smith, Jr.
Massachusetts Institute of Technology

Larry Smith
Microelectronics & Computer
Technology Corp.

Nancy J. Tighe
Microphase Systems Corp.

Theodore Van Duzer
University of California, Berkeley

Harold Weinstock
Air Force Office of Scientific Research

120

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Chapter 1

Summary

During 1987, high-temperature superconductivity (HTS) became a symbol—of the new and unexpected, of what was right and wrong in U.S. science, technology, and industry, of U.S.-Japan competition in high technology. In December of the preceding year, two scientists at IBM's Zurich research laboratory caught the world's attention with their discovery of superconductivity in the range of 35 to 40 °K (degrees Kelvin, i.e., degrees above absolute zero)—nearly double the record temperature for total loss of resistance to electricity. Within 2 months, transition temperatures had doubled once more—to over 90 °K—with near-simultaneous discoveries of a second family of ceramic superconductors in the United States, China, and Japan.

In March 1987, thousands of scientists jammed a hotel ballroom in New York to hear the latest findings—a meeting dubbed the Woodstock of physics. The race to higher temperatures was on. With it came warnings that the United States could lose out to foreign competitors in commercializing a technology with potentially revolutionary impacts. Indeed, one of the principal findings of this assessment is that **American companies may already have begun to fall behind**. Japanese firms have been much more aggressive in studying possible applications of HTS, and have more people at work, many of them applications-oriented engineers and business planners charged with thinking about ways to get HTS into the marketplace.

In the midst of the excitement, four congressional committees—the House Committee on Science, Space, and Technology, and the Senate Committees on Governmental Affairs, Energy and Natural Resources, and Commerce, Science, and Transportation—asked OTA to examine a series of questions that ranged from public and private sector responses to HTS (here and abroad) to the advantages and disadvantages of a new Federal agency for supporting the development of commercial technologies.

This special report begins with a look at U.S. strengths and weaknesses in technology development and commercialization (ch. 2), both in general and for HTS. The analysis then goes on (in ch. 3) to the strategies of U.S. and Japanese companies, as managers in each country look ahead to the new opportunities. The fourth chapter presents 20 policy options for congressional consideration; the context is U.S. technology policy as a whole, with HTS as a special case. Most of the policy options deal, in one way or another, with the management of the Federal R&D budget. Chapter 5, the last, considers three broad alternatives for speeding commercialization.[1]

COMMERCIALIZATION: PUBLIC AND PRIVATE DIMENSIONS

U.S. competitiveness in both smokestack and high-technology industries has been slipping for years. Loss of technological advantage has been one of the reasons (box A). On the face of it, this seems paradoxical. The U.S. Government spends more on R&D than government and industry together in Japan. Federal R&D dollars help create a vast pool of technical knowledge that the private sector (including foreign firms) can draw upon. Beyond this, U.S. technology policies have relied heavily on indirect incentives for innovation and commercialization by industry.

This approach — leaving R&D priorities largely to the mission agencies, trusting to in-

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[1] App. B, at the end of this report, summarizes the technology of superconductivity, including prospective applications, with estimates of time horizons for commercialization. (The glossary in app. A includes many of the specialized terms that apply to superconductivity.) OTA will follow this special report with a more detailed examination of the science and technology of superconductivity, the research agenda, and potential applications, to be published in 1989.

Box A.—The U.S. Position in Technology

Before World War II, U.S. technology was seldom better than foreign technology. Sometimes it was inferior. After the war, American industry took a decisive lead in applications of technical knowledge. Even so, the United States often found itself developing or adapting technologies that had originated elsewhere. Examples include the jet engine, ballistic missiles and satellites, and, more recently, a host of innovations in the automobile industry (radial tires, anti-skid braking systems, viscous-coupled four-wheel drive).

Like Japanese and European firms, American companies have always been imitators and adaptors, but in recent years the United States has neglected to tap the world's store of technical knowledge. Other nations grew economically, revived their scientific establishments, and regained their accustomed places in technology. Meanwhile, the sources of advantage that once bolstered the international competitiveness of U.S. industries narrowed:

- In sectors ranging from steel to automobiles to high-technology electronics, U.S. technology today is no better than foreign technology. Sometimes it is poorer, especially when it comes to process (rather than product) know-how. The problems are in technology, not in science: a healthy scientific enterprise has not been enough for the United States to maintain a useful lead in (non-military) technology.
- The United States still spends far more on R&D than any other market economy. But increases in the U.S. R&D budget have not been as rapid or steady as in Japan and several of the major European nations. Nor have resources devoted to commercial technology development (as opposed to defense) grown as quickly. By several measures, priorities for commercial R&D are lower in the United States than in Japan.
- A few hundred major corporations account for the great majority of U.S. industrial R&D. While some of these companies maintain central research laboratories for projects with longer time horizons, much of this work has been scaled back in recent years; most U.S. firms that conduct R&D limit their investments to projects promising rapid paybacks. American industry, by and large, looks for safe bets; few managers view research as a major element in long-term competitive strategy.

Given these circumstances, the policies followed for the past 40 years by the U.S. Government—reliance on military R&D and on funding for basic science to support the Nation's technology base—no longer appear adequate. Although military spending still leads to major new commercial products and processes, spinoffs are less likely today than in earlier years. The education of American engineers seems increasingly divorced from the realities of the marketplace and the factory floor.

Japanese and American firms start out even in the race to commercialize HTS.[1] Broadly speaking, Japan is behind the West in science; but superconductivity is one of Japan's research strengths. And in engineering, Japanese firms have long since proved their capabilities. In years past, Japanese companies began with foreign technologies and improved them; today, they compete quite effectively with home-grown know-how. If Japan were to surpass the United States in a new science-based technology like HTS, U.S. competitiveness could be very broadly threatened. The stakes have quickly come to seem a good deal greater than superconductivity itself.

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[1] Europe's ability to compete in high technology, despite unquestioned excellence in science and engineering, has continued to slip in the 1980s. Notwithstanding an array of new policy initiatives intended to reverse the trend, European industry will probably have great difficulty keeping up in HTS (see box D, ch. 2). This assessment, therefore, focuses on the United States and Japan.

direct policies to stimulate commercialization—worked well in the earlier postwar period, when American corporations were unchallenged internationally. On the evidence of steadily declining competitive ability across much of the U.S. economy, it no longer works well enough. In recent years, many U.S. companies have had trouble turning existing technical knowledge into successful products and processes, and getting new technology out of the laboratory and into the marketplace (ch. 2).

Of course there is more to commercialization than R&D and technology development. Government policies affect business decisions and competitiveness, not only through technology and science policy, but also through sector-specific measures (e.g., Government funding for the microelectronics consortium Sematech), and regulatory and macroeconomic policies. U.S. financial markets, for example, have been steadily deregulated. Among the results: greater pressures on industry for short-term investment decisions.

OTA has examined the broad range of policy influences on U.S. competitiveness in many other assessments. Here, the analysis focuses on those linked more or less closely to technology itself. They fall into two groups:

- policies that affect innovation and commercialization directly, notably the Federal R&D budget;
- those with indirect impacts.

Federal R&D helps create a technology base that private firms draw on during commercialization. Sometimes companies start development projects because of new research results; other times, they find they need critical pieces of knowledge, perhaps from earlier R&D, to complete a project, or to solve a manufacturing problem. Federally funded projects in low-temperature superconductivity (LTS), for example, laid the foundation for applications of superconducting magnets in medical imaging equipment.

The second group of policies works indirectly—through incentives (or disincentives) for private firms. Some of these policies reduce financial or technical risks, or increase rewards for successful innovators. Tax treatment of capital gains, for instance, affects decisions by prospective entrepreneurs; R&D tax credits make a difference for companies with profits that can be offset. Other such policies work through their influence on demand. Governments purchase military systems and computers, cars and trucks, consulting and construction services. Sometimes, they regulate prices or allocate production among suppliers (as the U.S. Government has done for years in agriculture).

With the knowledge base ever larger and more specialized, the great majority of American firms, large and small, can no longer expect to be self-sufficient in technology. The pace and complexity have simply outstripped their ability to keep up. Industry depends more heavily than ever before on the huge Federal R&D budget—$60 billion, about half of all U.S. R&D spending. Nonetheless, the U.S. Government has left most questions of R&D funding to the mission agencies, with their focused interests and immediate needs. While other countries have crafted policies for direct support of commercial technologies, the United States has not. Policymakers here have argued that direct measures lead to harmful economic distortions. Instead, many say, deregulation—removing the roadblocks to innovation—will tap reservoirs of American ingenuity and entrepreneurial vigor that would otherwise be stifled. But most of the roadblocks have come down over the past 15 years, while U.S. competitiveness has continued to slip.

To be sure, Federal agencies are paying more attention to the impacts of day-to-day decisions on competitiveness than during the 1970s. Antitrust enforcement reflects global, rather than simply domestic, competition. The national laboratories—particularly those overseen by the Department of Energy (DOE)—have been seeking ways to work more effectively with industry. With recognition spreading that military R&D spending may not offer the spinoffs and synergies of earlier years, Congress has been debating the merits of a change in direction for technology policies. But it is fair to say that international competitiveness still plays a minor role in U.S. policies compared with those of countries that have learned to export as effectively as Japan, West Germany, or South Korea. The United States is still searching for workable approaches to competing in a relatively open international economy, one in which American companies no longer have big advantages in technology or management skills.

HIGH-TEMPERATURE SUPERCONDUCTIVITY: U.S. AND JAPANESE RESPONSES

Why all the excitement over HTS? The media have held out the promise of more efficient generation and transmission of electric power, magnetically levitated trains, electromagnetic launchers for space weaponry. Perhaps more important, HTS-based electronics could eventually become building blocks for more sensitive medical diagnostic systems, and faster, more powerful computers. The most important impacts will probably be those that cannot yet be anticipated—the point may be facile, but it is true.

Even at liquid nitrogen temperatures—far below room temperature but far above the operating temperatures of older LTS superconductors—the prospects have attracted as much attention as any scientific development since the laser or gene splicing. Although no one had made a practical conductor or electronic device from the new materials, the Nobel Prize committee gave its 1987 physics award for the Zurich discoveries—the quickest in history. Early 1988 saw the discovery of several more families of HTS ceramics. Yet the ultimate prize—superconductivity at room temperature—lies ahead, and no one knows whether it can be achieved, even in theory.

Activity has been feverish on the policy front as well as in the research laboratory. Within a few months of the initial discoveries, Federal agencies redirected $45 million in fiscal 1987 funds from other R&D to HTS (ch. 4, table 8). The scientific breakthroughs prompted a dozen bills during the first session of the 100th Congress, proposals ranging from study commissions to a national program on superconductivity. All reflected, in one way or another, concern over commercialization.

The policy drama reached a peak in July 1987, when President Reagan brought three ranking cabinet officers to the Federal Conference on Commercial Applications of Superconductivity; in an unprecedented appearance, he announced an 11-point initiative for the support of HTS (box B, ch. 2). In a similarly unprecedented move, the Administration closed the meeting to all foreigners except representatives of the press. Although the President's message focused on executive branch actions, he stated that the Administration would also be proposing new legislation.

The following months brought a sense of anticlimax, with no sign of the promised legislative package. Questions of R&D funding then came to the fore, as the end of the fiscal year passed with no resolution of the budget impasse between the President and Congress. Only at the end of the calendar year—several months into fiscal 1988—did Federal agencies know for certain how much money they would have for HTS R&D.

Taken together, Federal agencies will spend nearly $160 million for superconductivity R&D in fiscal 1988, over half ($95 million) on the new materials (and the rest for LTS). The Department of Defense and the Energy Department together account for three-quarters of the HTS budget, and received most of the increase. DOE, for instance, will have nearly twice as much HTS money as the National Science Foundation (NSF). With NSF a primary patron of university research, the government's priorities seemed rather haphazard, given the great strength of the Nation's universities in basic research. Most of the Federal HTS money will go to government laboratories, contractors, and universities that are well removed from the commercial marketplace.

The President's legislative package, which reached Congress in February 1988, did not address R&D funding. Consistent with the Administration's emphasis on indirect incentives for commercialization, the package included provisions that would further liberalize U.S. antitrust policies, and extend the reach of U.S. patent protection.

On the industry side, most American firms—viewing payoffs from HTS R&D as uncertain and distant—have declined to invest heavily (ch. 3). A few major corporations—e.g., Du Pont, IBM, AT&T—are mounting substantial efforts. A number of small firms and venture startups have also been pursuing the new technology. By and large, however, American companies have taken a wait-and-see attitude. They plan to take advantage of developments as they emerge from the laboratory—someone else's laboratory—or buy into emerging markets when the time is right. Unfortunately, reactive strategies such as these have seldom worked in industries like electronics over the past 10 to 15 years, while many American firms seem to have forgotten how to adapt technologies originating elsewhere.

Corporate executives in Japan, in contrast, see HTS as a major new opportunity—one that could set the pattern of international competition for the 21st century. Japanese companies have made substantial commitments of people and funds, pursuing research and applications-related work in parallel. Firms in more lines of business are at work than in the United States. Steel companies and glassmakers, as well as chemical producers and electronics manufacturers, are seeking new businesses, ways to diversify. Japanese managers see in HTS a road to continued expansion and exporting, and are willing to take the risks that follow from such a view.

For years, the claim was common that Japanese firms got a free ride from U.S. R&D. More recently, Americans have realized that Japanese corporations have no need to imitate or to be followers; they have highly competent and creative technical staffs, fully capable of keeping up or taking the lead in fields ranging from automobile design to gallium arsenide semiconductors, opto-electronics, and ceramics. Giving the Japanese the credit they deserve has intensified U.S. anxieties over commercialization. Only in science—in basic research—do Japan's capabilities remain in question. For the Japanese, HTS presents an opportunity to show the world—and themselves—that they can be leaders there too.

Companies like IBM and Du Pont—or Hitachi and NEC—have R&D budgets exceeding a billion dollars. They have skilled engineers and scientists to put to work on the technical problems of HTS, money to bet on new opportunities. But these firms are a small minority in both countries, and the competition will not depend on them alone.

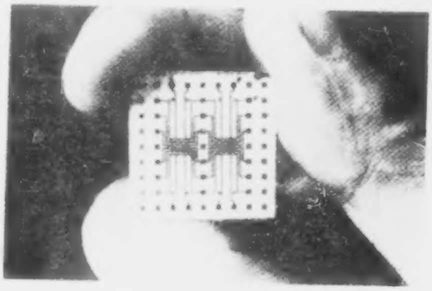

Photo credit: IBM

Conducting strips of HTS material deposited on substrate

PRINCIPAL FINDINGS

Federal Funding for HTS R&D

It would be hard to criticize the magnitude of U.S. Government spending on HTS. Federal agencies have about $95 million for HTS R&D in fiscal 1988, more than twice the 1987 total. Although little of this represents new budget authority, the U.S. Government will spend more this year on HTS than Japan's Government has budgeted for HTS and LTS together.

With the Administration seeking $135 million for HTS in fiscal 1989, current and proposed spending might seem more than enough to support rapid commercialization. But totals can be misleading. After all, the United States spends far more on R&D than competing nations, yet U.S. industry has been unable to keep a useful lead in technology. There are many reasons, some of them having to do with the allocation of R&D funds. Nearly 70 percent of

Federal R&D spending goes for national defense; some of this money helps build the technology base for commercial industries, some does not. The story will be the same in HTS.

1. Of $95 million that the U.S. Government has budgeted for HTS R&D during fiscal 1988, the Department of Defense (DoD) will spend $46 million and DOE $27 million. NSF is next at $14.5 million. No other agency has more than about $4 million. **R&D funded by DoD and DOE will help support commercialization, but a dollar spent by one of these agencies will probably buy substantially less in terms of the Nation's technology base than a dollar spent by NSF**.

   A good deal of DoD's R&D will go for specialized applications in defense systems—including the Strategic Defense Initiative—with limited potential for commercial spinoffs; defense missions shape even the basic research supported by the Pentagon. DOE will distribute most of its money to the national laboratories; relationships between DOE laboratories and the private sector have begun to change—a trend to be applauded—but the laboratory system has yet to demonstrate the ability to transfer technologies rapidly and effectively to the private sector. (See Policy Options 1, 2, 4 in ch. 4, and discussion in chs. 4 and 5.)
2. While the Federal R&D total for HTS may seem impressive, little of it represents new money. This was necessarily the case in fiscal 1987, when agencies had no choice but to redirect existing funds. For fiscal 1988, given the pressures on the Federal budget, agencies have continued to take money from other R&D categories to pay for HTS. **Congress may wish to examine the trade-offs necessary at the agency level to finance HTS R&D, and consider appropriating new money for fiscal 1989**. (Options 1, 2, 3, 9.)
3. R&D priorities and funding decisions—often made at relatively low levels in the agencies—have major and lasting impacts on commercialization. So do mechanisms for inter-agency coordination. The pressures on the Federal budget make good management of agency resources even more important. (Options 1, 2, 3, 6, 8.)

   But getting the most out of the Federal investment in HTS R&D will take more than inter-agency coordination and effective technology transfer. **Successful commercialization will require continuity in R&D funding so that people and organizations can plan ahead**. The United States will need graduate-level scientists and engineers educated in fields ranging from materials processing to the physics of electron devices. Most of these people get their training in university programs that depend heavily on Federal support. Likewise, the national laboratories and Federal mission agencies must know where they are going, and how much money they can expect along the way. Industry needs to know whether and when it can look for new research results from Federal R&D. Multiyear R&D planning and budgeting for HTS, on a trial basis, could help set patterns for the future. (Options 2, 3, 4, 5.)

R&D and Commercialization

No one can say whether superconductivity at room temperature will be possible in the near future or in the distant future. Regardless of progress in finding materials with higher superconducting transition temperatures, 5 to 10 years of R&D probably lie ahead before the technology base will be able to support substantial commercial development.

Successful commercialization, in any case, takes more than R&D. It depends on market conditions—on a company's ability to anticipate or create demand, and to exploit it. Linking engineering development, marketing, and manufacturing—something Japanese companies excel at—is crucial. So is management commitment to the long term.

1. Processing and fabrication methods will be critical for applications of HTS. American companies have fallen down in manufacturing skills across the board; the more heavily process-dependent HTS turns out

to be, the more difficult it will be for U.S. firms to keep up with the Japanese. **A strong processing emphasis in Federal R&D could help compensate for low priorities in American corporations, a major source of U.S. competitive difficulty**. (Options 2, 6, 15, 16 in ch. 4.)

The Defense Advanced Research Projects Agency (DARPA) solicited 200 proposals on HTS during the summer of 1987, hoping to have $50 million to spend on processing-related R&D. When the final 1988 budget figures came down (in December 1987), DARPA found itself with only $15 million. Nonetheless, even at this lower level the program should be able to make a substantial contribution to commercialization, if well managed and sustained over a number of years. (As this report went to press, the Defense Department had just imposed a freeze on new outside R&D, including this program.)

2. HTS R&D funded by defense agencies will help American companies, but the potential for commercial spinoffs will diminish as military requirements become more specialized and diverge from commercial needs. The list of new technologies and new industries that has emerged from DoD-sponsored R&D is an impressive one: computers; semiconductors; lasers; much automated manufacturing know-how. Why should things be any different with HTS? Because both the United States and the rest of the world have changed. The defense sector has grown apart from the rest of the U.S economy; DoD money has less impact as other countries focus more of their resources, both public and private, on commercial technologies. At the least, **continuing attention to technology transfer from defense contractors and Federal laboratories will be necessary to take commercial advantage of DoD (and DOE) spending**. (Options 11, 12, 13, 14, 15.)
3. Just as for technologies like microelectronics, commercializing HTS will require contributions from many disciplines—physicists, chemists, materials scientists, electrical, electronic, and chemical engineers. Multidisciplinary research works in industry because it must, but does not come easily in universities (here or in other countries). **Federal policies that help establish multidisciplinary R&D within the university system will contribute to strong foundations for HTS** and other technologies. (Options 9, 10.)

   NSF has embarked on a renewed attempt to stimulate multidisciplinary R&D through its program for Engineering Research Centers, and its proposed Science and Technology Centers. Consistent support will be required for these centers to take hold and become a permanent feature of the R&D landscape.
4. HTS will demand a good deal of trial-and-error development (as was true in LTS). With U.S. difficulties in commercialization much more a matter of technology than science, **Federal policies that increase support for engineering research—even more, that seek to redirect research and education in engineering toward practical industrial problems—could have substantial long-term significance**. (Options 4, 5, 9, 19.)

HTS in the United States and Japan

Japan's Government took the better part of a year to shape its policy response to HTS—a response that, when it emerged, looked not at all like the highly centralized program some Americans had expected. Much of the effort has been directed at getting the three parts of the R&D system—industry, the universities, Japan's national laboratories—to work effectively together. The Japanese see HTS as a test case for their turn toward basic research, and are giving it high priority. Moreover, lacking energy reserves, they have strong incentives for R&D (in LTS as well as HTS) promising savings in electric power consumption.

Japanese firms compete aggressively at home and abroad; they get consistent government support—for instance, from national laboratories that work effectively with the private sector—but succeed in international markets on their own merits. In some if not all industries, Japanese companies turn R&D into new products and processes faster than American firms. They target markets effectively, linking R&D to market needs better than many U.S. companies, and manage their factories at least as well as they manage their R&D laboratories. These strengths will pay off in HTS.

1. A few large American companies are putting substantial resources into HTS. But the list is short: AT&T, IBM, Du Pont, a few others. The financial criteria that drive decision-making in American corporations work against a technology like HTS—one with uncertain prospects, and profits that lie well in the future; **the short-term view fostered by U.S. financial markets could put American companies behind the Japanese within 2 or 3 years**, if they are not behind already.

   A handful of small U.S. companies and startups with venture funding have also been moving into HTS. Although smaller U.S. firms may well develop creative solutions to some of the practical problems of the new technology, these companies do not have the production and marketing capabilities necessary for a major role. They will have a difficult time growing and competing with integrated Japanese multinationals.
2. American managers, by and large, believe HTS should remain in the laboratory until more scientific knowledge is in hand. They emphasize the uncertainties—admittedly great—and the lack of evidence promising quick returns from R&D investments. To them, uncertainty urges caution rather than signifying opportunity. **American firms have not made commitments to HTS that compare to those in Japan in terms of scale (as indicated by people at work) or scope (as indicated by people assigned to applications-related projects).**

   Many American companies with the technical skills and the money to pursue HTS have taken a wait-and-see attitude. Typically, they have a few people tracking progress in the field. Some of these companies may be able to catch up and compete when applications begin to appear. Others will be left behind.
3. Most Japanese managers believe HTS to be closer to the marketplace than do their American counterparts. Seeking growth and diversification, they have assigned more people to HTS than U.S. firms, and may also be spending more money. The Japanese have committed funds, not only to research, but to evaluating prospective applications. Executives there see HTS as a vehicle for creating new businesses, while Americans are more likely to view it in terms of existing lines of business. And if American managers have been reluctant to commit resources to HTS, **the Japanese seem confident that investments now will pay off**—some time and in some way.

   The Japanese could be wrong. In spending money on feasibility studies and engineering analyses, they may miss other opportunities. But given the scale of current investments—in the range of $200 million dollars in each country (including both government and industry R&D), small compared to overall corporate R&D spending—there is much to be said for taking the risks. OTA's analysis suggests that commercialization of HTS will proceed somewhat faster than many American managers anticipate, though not so fast as many in Japan expect. If this proves the case, **Japanese companies could well come out ahead in the race to commercialize HTS**.
4. Japan's Government will spend about $70 million for superconductivity R&D (high temperature and low) in 1988.[2] Although ministries and agencies spent much of 1987 jockeying for position, Japan now has in place a set of policies intended to compensate for the bottlenecks and weaknesses in the country's R&D system: universities with only a few islands of excellence; national laboratories which, although some

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[2] Comparisons with U.S. Government spending must be treated with caution: fiscal years in the two countries are 6 months out of phase; Japanese budget figures leave out salaries for research workers in universities; national defense has little influence in shaping Japan's HTS R&D.

have enviable reputations, cannot claim the breadth or depth of their U.S. counterparts.

If their system as a whole still shows weaknesses, **in superconductivity, Japan's R&D is broadly based and high in quality**. With R&D centered in major corporations, government policies aim to strengthen the infrastructure for developing HTS, and stimulate greater cooperation and interaction among industry, universities, and the national laboratories.

5. Japanese officials view international cooperation in HTS research as a potential complement to their country's own efforts. Much more than a matter of image, they see in internationalization a means of stimulating creativity in Japan's universities and government laboratories. In turn, **U.S. industry stands to gain by testing Japan's willingness to open up its research system**. (Options 18, 19, 20 in ch. 4.)

HTS and U.S. Technology Policy

Japanese companies place high priorities on technology as a competitive weapon; it is not only in HTS that U.S. companies risk falling behind. Business-funded R&D in Japan totals 2.1 percent of gross national product, compared with 1.4 percent here. Fewer high-level managers in American firms have technical backgrounds; they may not fully appreciate the role of R&D in business strategy and international competition. To executives fighting a takeover, research may look like a luxury; after a merger, it may seem expendable.

Gaps in the U.S. technology base open where neither Government nor industry has immediate requirements for R&D results. The very unexpectedness of the discoveries in HTS points to the need for ongoing Government support of long-term research. Failure by the private sector to invest in generic R&D, much of it incremental, or in risky projects with potentially big payoffs, throws more of a burden on the Federal Government.

1. **Many areas of science and technology, although vital for U.S. competitiveness, get adequate financial support from neither public nor private sources**. American corporations have been turning away from long-term, high-risk R&D—the kind of work called for in commercializing HTS. Knowledge that could help American firms compete is not available when needed. Underinvestment has been most serious in fields that lack glamour—e.g., manufacturing. (Options 6, 7, 8, 15, 16 in ch. 4.)

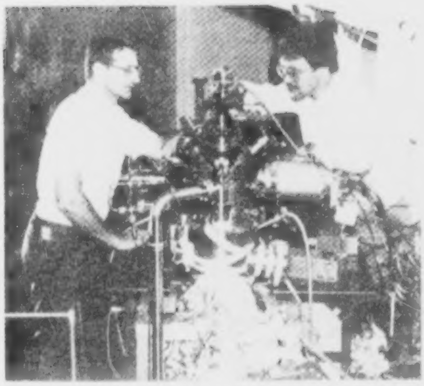

Photo credit: National Bureau of Standards

Synchrotron ultraviolet radiation equipment, used for studying electronic structure of HTS materials.

2. Like industry, the Federal Government spends most of its R&D dollars on development. Government money goes primarily for mission-oriented projects. When Federal agencies pay for R&D on civilian, commercial technologies, they have often made poor choices—particularly when the R&D goals are well removed from agency missions. **Without substantial changes in U.S. technology policies, industry can expect only limited help from the Federal Government in the commercialization of HTS and other new technologies**. Recently, some State Governments have been more active than the Federal Government in stimulating industrial technology development. (Options 6, 8, 15, 16, 17.)
3. Federal funding aimed at filling gaps between fundamental research and product/process development could help speed

utilization of new technologies, including HTS. Funding for long-term, high-risk projects with potential commercial applications could be even more important. But policies during the 1980s have been moving in the opposite direction; with the Administration cutting budgets for civilian applied research, the overall thrust of U.S. technology policy has turned away from support for commercial R&D. Instead, the Government has relied on indirect measures for stimulating industrial innovation and technology development. OTA's analysis suggests that **the indirect approach, emphasizing measures such as looser antitrust enforcement and stronger patent protection, does not, by itself, go far enough**.

Direct support for commercial technologies has never had a fair trial in the United States. Indirect measures certainly have a place: for example, incentives for corporate basic research could help U.S. competitiveness. So could a supportive climate for cooperative R&D ventures (and, perhaps, Federal cost sharing). Even so, given that policies such as the R&D tax credit (in place since 1981) have had little apparent effect in filling the holes in the Nation's technology base, it seems at least as important for the Federal Government to reconsider direct funding of applied industrial R&D.

When it comes to commercial technology development, the needs are two-fold:

- **support for generic, pre-competitive technologies—those that can help a wide range of companies compete more effectively, without giving any one of them a big advantage; and**
- **support for long-term, high-risk projects**.

Much of the generic R&D would be relatively straightforward—incremental research with a strong engineering focus. The long-term, high-risk thrust could be modeled to some extent on the work DARPA undertakes for the military.

FEDERAL GOVERNMENT STRATEGIES

The last chapter of this report discusses three strategies for commercialization. These strategies imply choices going well beyond the individual policy options referred to above and discussed in detail in chapter 4—most of which are discrete and relatively narrow.

The first of the three strategies—flexible response—the current, *de facto* approach, builds on the proven strengths of the U.S. system. These strengths include diversity in funding and conducting R&D: NSF, with its mandate for financing high-quality university research regardless of field; defense agencies, with their unmatched budgets; national laboratories, reservoirs of skilled professionals.

Despite its acknowledged strengths, the flexible response strategy seems unlikely to provide adequate support for HTS. The funding picture summarized above for HTS and discussed in detail in chapter 4 shows the drawbacks of the flexible response approach. Most of the Federal dollars for HTS will go to mission agencies with little experience in commercialization—to DoD and DOE. NSF—primary sponsor of untargeted university research in science and engineering—has not had the money to fund many of the highly rated proposals it has received. No one in Government has an overview of Federal support for HTS. Few mechanisms exist for debating and determining priorities.

Congress could, of course, choose a more aggressive response to HTS—the second of the three strategies analyzed in chapter 5. Three elements set this strategy off from the current approach:

- more money to NSF for basic research on HTS (and perhaps for one or more interdisciplinary university centers), an insurance policy against missed opportunities;
- Federal Government cost-sharing in collaborative R&D programs organized and guided by industry (with the Federal money extending the R&D time horizons, ensur-

ing more support for generic work and high-risk research); and

- a working group of experts drawn from universities, industry, and Government to help shape consensus on HTS R&D priorities, and make decisions on Federal cost-sharing.

This second strategy would direct Federal funds into HTS R&D that might otherwise be underfunded, and particularly into industry. The added cost would be modest—**$20 million or $30 million per year, well spent, should make a big difference.**

The last of the strategies goes beyond HTS, taking up the question of direct Federal support for commercial technology development. As part of such a strategy, OTA considers the merits of increased funding for engineering research, along with the advantages and disadvantages of a Federal technology agency.

The analysis emphasizes the problems of defining an acceptable mission for such an agency—one charged with supporting industrial technologies—and of avoiding special-interest hand-outs. Without a mission statement that can impose discipline over the agency's decisions, both day-to-day management and the establishment of broad priorities pose real difficulties. Nonetheless, a Civilian Technology Agency might be able to provide useful support for commercialization if its activities were centered on generic R&D, intended to fill holes in the Nation's technology base, and on a menu of long-term, high-risk projects.

The three strategies in chapter 5 are by no means exclusive of one another. As Federal policies shift in response to the new competitive circumstances of American industry, and as the science and technology of superconductivity continue to evolve, Congress and the Administration—along with private industry—will need to remain flexible and open to new ideas. Technological innovation may demand policy innovation. Uncertainty makes planning difficult for both public and private sectors—one of the reasons for a strategic framework to aid in the many decisions that lie ahead.

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Chapter 2

Commercialization: Government and Industry

SUMMARY

The United States invents and Japan commercializes. So say some. Is it true? If so, this would suggest not only that American companies fail to capitalize on technologies developed here, but that Japanese firms get a free ride on American R&D. Furthermore, if this has happened in other industries and with other technologies, it could happen with high-temperature superconductivity (HTS).

Has American industry really had that much difficulty in commercialization—in designing, developing, manufacturing, and marketing products based on new technologies? Yes—in *some* industries and with *some* kinds of technologies. In other cases—for example, biotechnology or computer software—American firms continue to do better at commercialization than their overseas rivals. Nonetheless, the competitive difficulties of American semiconductor firms have long since shown that continuing U.S. advantages in high technology cannot be assumed. And sectors like consumer electronics demonstrate that, *when it comes to engineering, if not science, Japan has been a formidable presence since the 1960s.*

Commercialization is the job of the private sector. Government plays a critical role in two respects:

1. *R&D funding.* Federal agencies will spend some $60 billion on R&D in 1988. Government dollars create much of the technology base that companies throughout the economy draw on. In 1988, the U.S. Government will spend some $95 million on HTS R&D. This is about as much as the American firms surveyed by OTA say they will spend on superconductivity R&D (LTS as well as HTS) in 1988. (See ch. 3, box F).
2. *The environment for innovation and technology development.* A host of policies—ranging from regulation of financial markets, to protection for intellectual property, and education and training—affect commercialization by companies large and small.

Private firms use scientific and technical results—more or less freely available, including knowledge originating overseas—in their efforts to establish proprietary advantages. Universities and national laboratories create much of the science base. Some industrial research contributes to the storehouse of scientific knowledge. All three groups—universities, government laboratories, industry—contribute to the larger technology base (which includes science but goes well beyond it).

Much technical knowledge remains closely held—protected by patents, by secrecy (classification for reasons of national security, trade secrets), or simply as proprietary expertise. Much proprietary information resides in peoples' heads, in organizational routines, management styles, as tacit know-how. Companies also write down some of their organizational knowledge: in product drawings and specifications; in process sheets, manuals, and computer programs for running production lines and entire factories. The manufacturing skills that helped Japanese semiconductor manufacturers outstrip their American competitors depend heavily on proprietary know-how, much of it embodied in the skills of their employees—skills that people often cannot fully articulate or explain.

Commercialization of HTS will depend on scientific knowledge, much of this available to anyone who can understand it. It will also depend on know-how, hard-won learning and experience—making good thin films, orienting the grains in superconducting ceramics to increase current-carrying capacity. Knowledge of markets will count too.

Government contributes directly through support for the technology and science base.

Federal agencies may spend their HTS R&D budgets wisely, or not. National laboratories may transfer technologies to the private sectors quickly, or only after long bureaucratic delays.

Government policies also affect commercialization indirectly. Patents and legal protection of trade secrets help firms stake out proprietary technical positions. Education and training policies (and immigration policies) affect the labor pool from which companies hire people who can understand the science of HTS, envision new computer architectures based on superconducting electronics, grasp the market opportunities created by the new materials.

No one anticipated superconductivity at 90 or 125 K. No one can predict what will come next. More likely than not, 5 to 10 years of R&D—much of it supported by Federal agencies—lie ahead before HTS markets will have much size or begin to grow rapidly. A few niche products could come sooner. So could some military applications. New discoveries could change the picture radically. The ways in which the Federal Government spends its R&D dollars matter right now. Policymakers may have a bit more leisure to review the other channels of policy influence on commercialization of HTS. The stakes are high—for the private sector, and for government decisionmakers.

Potential for dramatic breakthroughs, coupled with great uncertainty, makes for difficult decisions. OTA sees no reason to rule out the possibility of room-temperature superconductivity (next month, next year). Room-temperature superconductivity—in a cheap material, easy to work with—has almost unimaginable implications. Companies with proprietary technical positions could reap huge rewards. The risks of inaction are high; on the other hand, progress could stall. High expectations and media hype could be followed by disillusionment, difficulty in raising capital, inaction on the policy front. Biotechnology has already lived through several such waves. HTS probably will too.

Early applications of new technologies tend to be relatively specialized, of modest economic significance. The public may lose interest, financial markets downgrade the prospects. No one can know, at this point, whether HTS could turn out to be a solution in search of a problem. The laser—invented in 1960—never seemed to live up to expectations. And yet solid-state lasers eventually made fiber-optic communications possible—an innovation with vast impacts on a worldwide scale (including, for example, a new source of competition for satellite communications systems). It was not that the possibilities went unrecognized.[1] Prospective applications of the laser to eye surgery and optical communications got immediate attention; but while ophthalmologists quickly began using lasers, little progress was made in communications for 15 years. It took, not only solid-state lasers, but low-loss glass fibers to make optical communications a reality.

In the early years of laser technology, no one fully anticipated the possibilities for fiber-optic communications networks; they snuck in through the back door. The same could happen with HTS. One of the tasks for public policy is to bring stability to the early years of new technologies, building a base for later commercialization. Industry will not do this alone, absent the potential for near-term profits.

OTA's analysis suggests that commercialization of HTS will proceed somewhat faster than many American companies expect, though not as fast as the Japanese companies that have been making heavy investments seem to anticipate. (Ch. 3 outlines U.S. and Japanese business strategies toward HTS.) As American companies move down the learning curves that mark out accumulated knowledge and experience in HTS, federally funded R&D will provide critical support for the technology base that all firms—but particularly smaller companies—draw from.

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[1] "The Maturation of Laser Technology: Social and Technical Factors," prepared for OTA by J.L. Bromberg, The Laser History Project, under contract No. H3-5210, January 1988, pp. 7-9. Theodore Maiman, who built the first laser in 1960, stressed the communications possibilities—multi-channel capability, low cost per channel—at the press briefing announcing his invention.

The analysis in this chapter leads to the following conclusions:

- Small, entrepreneurial firms will be well placed to develop commercial applications of HTS. The conditions are right: a new science-based technology; synergistic links with existing industries, including low-temperature superconductivity (LTS) and electronics; venture capital for good ideas. But while small companies have been a major source of U.S. strength in high technology, few can assemble the financing, the technological breadth, or the production and marketing capabilities to grow as fast as their markets.
- Larger American corporations may find that they are starting out behind some of their Japanese rivals. The new HTS materials are ceramics. Japanese firms have a useful lead in both structural and electronic ceramics. Some of this expertise will transfer to HTS. So will a good deal of know-how developed for fabricating microelectronic devices—another field where Japanese firms have demonstrated themselves to be at least as good and sometimes better than American companies.
- Processing and fabrication techniques will be critical for commercialization. American companies have fallen down in manufacturing skills across the board; the more heavily process-dependent HTS applications turn out to be, the more difficult it will be for U.S. firms to keep up with the Japanese.
- Product as well as process technologies will demand much trial-and-error development. Japanese engineers and Japanese corporations are good at this. American companies are not. To the extent that commercialization of HTS depends on step-by-step, incremental improvements—brute-force engineering—U.S. companies will be in relatively poor positions to compete.
- R&D funded by the U.S. Government will help American companies in commercializing HTS, but the spinoffs from defense-related R&D may not be large or long-lasting if military requirements become highly specialized and diverge from commercial needs.
- Indirect policy measures—intended to remove the roadblocks to commercialization and increase the rewards for innovators and entrepreneurs—can also help. But the indirect approach alone will not be an adequate response to the coming international competition in HTS.

What about U.S. commercialization in general—the backdrop for the statements above?

- Mobility among scientists, engineers, and managers has spurred rapid growth and technological innovation in postwar U.S. high-technology industries ranging from computers and semiconductors (starting in the 1940s and 1950s) to biotechnology (beginning in the late 1970s). Venture capital for small, high-technology firms, likewise, has been a consistent source of competitive strength, one that will continue to work to U.S. advantage in HTS.
- Many larger American companies have pulled back from basic research and riskier technology development projects. Ease in establishing new small firms compensates in part for these relatively conservative investment decisions; indeed, negative decisions on proposed R&D projects sometimes spawn startups that go on to commercialize new technologies. Some of this will probably happen in HTS.
- With few American firms self-sufficient in technology, a lack of long-term R&D in the private sector, and managements that look for home-run opportunities rather than building technologies and markets step-by-step, the Federal Government has, by default, become a primary source of support for technology development. As yet, agency missions do not reflect this new role.
- Despite the onslaughts of foreign firms since the late 1960s, many American companies have not yet made the changes in their own organizations necessary to compete more effectively. Paying little more than lip service to well-known engineering methods such as simultaneous prod-

uct and process design, they fail to give manufacturing high priority. Neither managers nor engineers in the United States have learned to take advantage of technologies originating overseas.

- Industry cannot justifiably blame the U.S. Government for its failures. Compared with most other industrial economies, U.S. policies create a favorable environment for innovation and commercialization.

The indirect policy approach the U.S. Government has traditionally relied on to stimulate innovation and commercialization worked well for many years. Today, with foreign competition stronger than ever before, it seems time to explore new directions. The Federal R&D budget has grown rapidly over the postwar period. Management practices in government agencies, mechanisms for setting priorities, for ensuring an adequate technology base, have not kept pace.

The climate for innovation can always be improved, the barriers reduced. But the barriers are low already, and limited scope remains for policies intended simply to unleash American industry to compete more effectively. Indeed, the short-term perspectives of U.S. corporations, many of which have been unwilling to keep pace with foreign investments in new technologies, stem in part from the removal of another set of barriers—deregulation in U.S. financial markets.

Unless the United States learns to match the kinds of supports for commercialization that have proven effective elsewhere—topics treated in more detail in later chapters—only small improvements can be expected. U.S. industry could fall behind in HTS, and in the uses this new technology will find.

THE GOVERNMENT ROLE

HTS is fresh from scientific laboratories, but many commercial innovations begin with existing knowledge, gleaned from textbooks, design manuals, the schoolhouse of experience. The work of commercialization centers on engineering: development of new products and new manufacturing processes. Companies support their development groups with marketing people, and in some cases with research. Sometimes new science is part of commercialization, but not always.

The process may begin with an idea that is old, but has never been reduced to practice because of gaps in the technology base. The automobile, the airplane, and the liquid-fueled rocket all had to await needed pieces of technical knowledge. The Wright brothers learned to steer and stabilize their flying machine. Despite years of trial and error (and centuries of speculation), they were the first to find a way around these technical barriers.

Superconductivity itself, discovered in 1911, has a long history as a specialized field of physics, and a shorter history—beginning about 1960—as a technology that private firms sought to exploit. Appendix B, at the end of this report, summarizes the science and technology of superconductivity at both low temperatures (e.g., where liquid helium commonly provides cooling) and high (above the boiling point of liquid nitrogen).

Support for Industry: Direct and Indirect

What does this have to do with government? Today, governments finance much of the R&D that provides the starting point for commercialization. Companies everywhere start with this publicly available pool of technical knowledge in their search for proprietary know-how and competitive advantage. Second, public policies influence the choices companies make in financing their own R&D, and in using the knowledge available to them. Tax and regulatory policies encourage or discourage investments in commercial technology development. Patents create incentives, high capital gains taxes disincentives.

Smaller companies depend heavily on externally generated knowledge; many manufacturing firms with hundreds of employees have few if any engineers on their payrolls. But if smaller companies have the greatest needs, science and technology move so fast today that big companies also rely heavily on government R&D. Moreover, pressures for near-term profits have forced many larger U.S. corporations away from basic research. In the United States, a few hundred large companies account for the lion's share of industry-funded R&D—three firms (IBM, AT&T, General Motors) for more than 15 percent.

Half of all U.S. R&D dollars come from the Federal treasury. The fraction is smaller in most other countries, but in all industrial economies public funds pay for a substantial share of national R&D. The reasons begin with health and with national defense, but competitiveness has been one of the rationales: the first government research laboratories, established in the early years of this century in Britain, Germany, and the United States, were intended to help domestic industries meet foreign competition.

Foreign firms have access to many of the results of federally funded R&D, just as U.S. firms can tap some of the technical knowledge generated with foreign government support. Governments seek to use technology policy to help domestic firms compete, while commercial enterprises seek to take advantage of the world store of technical knowledge. Technology policy begins with R&D spending—setting broad priorities, making funding decisions at the project level, agency management. Other tools include intellectual property protection, which can help domestic firms establish and protect a technological edge. Of course, many countries also provide direct funding for commercially oriented R&D.

The U.S. Position in Technology

Past OTA assessments have examined U.S. competitiveness in a number of industries, and linked technological position with competitiveness; the most recent found signs of slowdown in U.S. R&D productivity, as well as evidence that newly industrializing countries have made surprising gains in technology.[2] Principal findings from these earlier assessments include:

- Technology is vital for competitive success in some industries (including services like banking). In others, it may be secondary. But in all or nearly all sectors, the technological advantages of American firms have been shrinking for years. The United States may be able to retain narrow margins in some technologies. Parity will be the goal in others. Regaining the advantages of the 1960s will, in the ordinary course of events, be impossible.
- In newer technologies, those that have developed since the 1960s, the Japanese have been able to enter on a par with American firms, and to keep up or move ahead. Examples include optical communications, and both structural and electronic ceramics. European firms, in contrast with the Japanese, have had trouble turning technical knowledge into competitive advantage.
- Today, U.S. military and space expenditures yield fewer and less dramatic spinoffs than two decades ago. The U.S. economy is vast and diverse. Defense R&D—increasingly specialized when not truly exotic—cannot provide the breadth and depth of support needed for a competitive set of industries.
- Japan and several European countries place higher priorities on commercial technology development than does the United States. R&D spending by Japanese industry reached 2.1 percent of gross domestic product in 1986, compared with 1.4 percent here.

Productivity, Innovation, Competitiveness, Commercialization

Import penetration in steel and consumer electronics, going back two decades, marks the beginnings of the wave of concern over lagging

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[2] *International Competition in Services* (Washington, DC: Office of Technology Assessment, July 1987), ch. 6. Also see "Development and Diffusion of Commercial Technologies: Should the Federal Government Redefine Its Role?" staff memorandum, Office of Technology Assessment, Washington, DC, March 1984.

U.S. productivity growth and competitiveness. Commercialization is simply the latest catch phrase for problems that are all of a piece. The ongoing policy debate has centered on the proper mix of policies in the United States, where government has been reluctant to intervene as directly or as deeply in the affairs of industry as, say, in Japan or France.

During the Carter Administration, an interagency task force, supported by a panoply of private-sector advisory committees, labored for 18 months to produce a Domestic Policy Review of Industrial Innovation (DPR). The recommendations included:[3]

- easier licensing of federally owned patents;
- stronger ties between universities and industry;
- help for small, entrepreneurial firms through small business innovation research grants;
- removal of unnecessary regulatory barriers;
- signals to industry that antitrust policy did not bar cooperative R&D;
- tax incentives for R&D and innovation.

Plainly, the focus was on indirect policies. In one form or another, most of these steps have been taken.

Other recommendations of the Carter DPR, dealing with direct support for technology development, were not implemented. After Congress passed the Stevenson-Wydler Technology Innovation Act in 1980, the Reagan Administration declined to act on the central provisions of the legislation, which called for a network of Centers for Industrial Technology charged with supporting commercial technology development.[4]

The Reagan White House began its own study of the problems in mid-1983, creating a Commission on Industrial Competitiveness headed by John Young, president of Hewlett-Packard. When the Commission delivered its findings a year and a half later, many of the recommendations were familiar: "balance" in regulations; better labor-management cooperation; stronger protection for intellectual property.[5] Although its leadoff recommendation called for a new Department of Science and Technology (which got a frosty reception from an Administration committed to scaling back the Federal bureaucracy), the Young Commission, like the Carter DPR, stressed the indirect influences of Federal policies on technology development. The Commission helped turn the spotlight on technology transfer from the national laboratories, and urged use of the tax system to encourage private-sector R&D.

During the 1980s, then, the environment for technology development continued to evolve along the lines mapped out by the Carter DPR. Congress included an R&D tax credit in the 1981 tax bill, and extended it—although at a lower level—in 1986. In 1982, Congress passed the Small Business Innovation Development Act, requiring Federal agencies to set aside 1.25 percent of extramural R&D budgets exceeding $100 million for awards to smaller companies. With the executive branch adopting a much-relaxed enforcement policy for antitrust, the National Cooperative Research Act of 1984 explicitly permitted certain forms of joint private-sector R&D, while limiting private antitrust suits to actual (rather than treble) damages. The Administration also began negotiations with the governments of several foreign countries where pirating of U.S. intellectual property has been at its worst.

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[3] For a brief summary, see J. Walsh, "What Can Government Do for Innovation?" *Science*, July 27, 1979, p. 378, together with N. Wade, "Carter Plan to Spur Industrial Innovation," *Science*, Nov. 16, 1979, p. 800.

In addition to agency participants, several hundred people from outside government took part in the Carter DPR; for the reports of the private sector committees and subcommittees, see *Advisory Committee on Industrial Innovation: Final Report* (Washington, DC: Department of Commerce, September 1979).

[4] Section 6 of the Stevenson-Wydler Technology Innovation Act of 1980 (Public Law 96-480) directed the Secretary of Commerce to "provide assistance for the establishment of Centers for Industrial Technology." Section 8 extended this authority to the National Science Foundation. The centers were envisioned as supporting generic technologies at the pre-competitive stage—those that could benefit many companies and industries. Commonly cited examples included R&D on welding processes, or on steelmaking. See *Implementation of P.L. 96-480, Stevenson-Wydler Technology Innovation Act of 1980*, hearings, Subcommittee on Science, Research, and Technology, Committee on Science and Technology, U.S. House of Representatives, July 14, 15, 16, 1981 (Washington, DC: U.S. Government Printing Office); also *International Competition in Services*, op. cit., pp. 364-365.

[5] *Global Competition: The New Reality*, vols. I and II (Washington, DC: U.S. Government Printing Office, January 1985). Most of the 30 members of the Young Commission were businessmen

At the same time, Federal laboratories—particularly those funded by the Department of Energy (DOE)—were seeking tighter linkages and better working relationships with private industry. During the early 1980s, the Federal laboratory system had come in for some rather harsh scrutiny.[6] An outside review panel (headed by David Packard, one of Hewlett-Packard's founders and a former Pentagon official) called for closer interactions with the private sector, setting the stage for efforts still underway to open up the laboratories and place their relationships with industry on a new footing (ch. 4). Meanwhile, State Governments began taking more active roles in technology policy.

President Reagan's proposed Superconductivity Competitiveness Act (box B) continues the stress on indirect policies. The draft legislation—which would further relax U.S. antitrust policy, while extending the reach of patent protection—would apply quite generally to U.S. industry; there is little that is specific to HTS.

R&D Funding and Objectives

If the weight of explicit shifts in U.S. technology policy during the 1980s has been on the indirect side, the direct role of the Federal Government has also changed—though not in the direction of support for commercial technology development. Government R&D has grown under the Reagan Administration, but much of the expansion has been for defense. Support for commercially oriented R&D has lagged, and in many cases been cut back.

Department of Defense (DoD) R&D went from $20.1 billion in fiscal 1982 to $37.9 billion in 1988 (table 1). DoD R&D, plus the defense-related portion of DOE spending (about half the Department's R&D), account for nearly 70 percent of all Federal R&D (figure 1); the great majority consists of applied research and the engineering of weapons systems.

As figure 1 suggests, the U.S. Government has not paid much attention, relatively speaking, to R&D of interest to companies outside the defense, aerospace, and health sectors. And in the 1980s, Federal agencies have backed away even further (e.g., from energy R&D). The Reagan Administration has held that government has no business supporting commercial technology development. Fundamental research, yes, but anything more would be a subsidy—unjustified and likely to create harmful economic distortions.

The basic research portion of the DoD budget does contribute quite directly to the Nation's store of commercially relevant technical knowledge. The Pentagon, for example, provides nearly 40 percent of Federal support for university research in engineering.[7] In constant dollars, however, DoD basic research (budgeted at $892 million for fiscal 1988) remains at roughly the same level as in 1967, while the total DoD R&D budget has been steadily expanding in real terms.

Based on 1987 obligations, the Federal R&D budget breaks down as follows into the three broad categories of basic research, applied research, and development:

Basic research	$ 8.8 billion
Applied research	9.0 billion
Development	38.7 billion
	$56.5 billion

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[6] See, for example, P.M. Boffey, "National Labs Reel Under Criticism and Investigation," *New York Times*, Aug. 24, 1982, p. C1.

The Packard report, below, appeared as *Report of the White House Science Council Federal Laboratory Review Panel* (Washington, DC: Office of Science and Technology Policy, May 1983). For more recent perspectives, see F.V. Guterl, "Technology Transfer Isn't Working," *Business Month*, September 1987, p. 44, and E. Lachica, "Federal Labs Give Out Fruit of More Research For Commercial Uses," *Wall Street Journal*, Feb. 1, 1988, p. 1.

[7] The National Science Foundation follows, at about 30 percent. Universities carry out half of all DoD-sponsored basic research. See *Directions in Engineering Research: An Assessment of Opportunities and Needs* (Washington, DC: National Academy Press, 1987), pp. 46 and 63. Recently, the military has spent a little more than 2 percent of its R&D budget on fundamental research; 5 percent of private industry's R&D total goes for basic work.

Box B.—The Reagan Administration's Superconductivity Initiative

The stated objectives of the HTS initiative announced by President Reagan in July 1987 include cooperation among government, industry, and universities in research, enabling American firms to move quickly in turning scientific advances into new or improved products and processes. The President called for stronger intellectual property protection, and spoke of government as a catalyst, "funding basic research, sharing information, removing the impediments to commercialization."

The initiative also outlined roles for the major Federal R&D agencies. The Department of Defense was to have the lead in direct R&D support, spending nearly $150 million over 3 years; though not explicitly mentioned, the Administration has viewed the Strategic Defense Initiative as a major customer for HTS technologies. DOE's national laboratories were given three superconductivity research and information centers (without new funds); the Department has taken a number of other steps on its own. The Administration charged the National Bureau of Standards with specialized applications of HTS to electronic devices.

The initiative called on the Patent Office to accelerate review of applications dealing with superconductivity, and instructed officials to seek reciprocal opportunities for participation in R&D supported by Japan's Government. Finally, the President announced that the Administration would propose new legislation in three areas:

- amendments to the 1984 National Cooperative Research Act giving explicit permission for some kinds of joint production ventures;
- extending the coverage of process patents so that damages could be collected from importers bringing in goods made abroad with process technology covered in the United States;
- tightening control over access to scientific and technical information from Federal laboratories.

The Administration sent its draft legislative package—the Superconductivity Competitiveness Act of 1988—to Congress 7 months later, in February 1988.

Only the last of the three provisions has immediate implications for HTS, given that production is not yet in sight. The language would allow Federal agencies to reject requests made under the Freedom of Information Act for scientific and technical information from Federal laboratories that could be "reasonably expected to cause harm to the economic competitiveness of the United States." Nothing in the Administration's message suggested that the United States might have as much to lose as to gain from barriers to the flow of information.

Title II of the proposed Act is intended to ease the concerns of firms contemplating joint production ventures. U.S. antitrust law does not bar such ventures, which have been common. The proposal would simply extend the rule-of-reason and actual-damages-only protections now afforded joint R&D ventures under the National Cooperative Research Act to joint production ventures. Any near-term impact on commercialization of HTS seems doubtful.

The White House press release also suggested that the process patent provisions would stimulate U.S. firms to seek more efficient manufacturing technologies. Again, this seems doubtful, at least for HTS. It would be surprising to find U.S. firms curbing their investments in HTS R&D now for fear that foreign firms would, at some future time, ship HTS-based products to the United States that had been fabricated with technologies covered by U.S. process patents.

Many agencies subdivide these categories further.[8]

Basic research itself covers a wide range of activities. Some of this really is "untargeted" science—work that could be called pure research. Nobody expects that astrophysics or the

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[8] No figures for 1988 were available as this report was being completed. Distinctions between these categories are necessarily arbitrary; for the Federal Government definitions, see *Science Indicators: The 1985 Report* (Washington, DC: National Science Board, 1985), p. 221. DoD subdivides its R&D budget into six subcategories, designated as follows:

6.1 Research
6.2 Exploratory Development
6.3 Advanced Development
6.4 Engineering Development
6.5 Management Support
6.6 Operational Systems Development

Several of these are further subdivided.

Table 1.—R&D Budget by U.S. Government Agency, 1988[a]

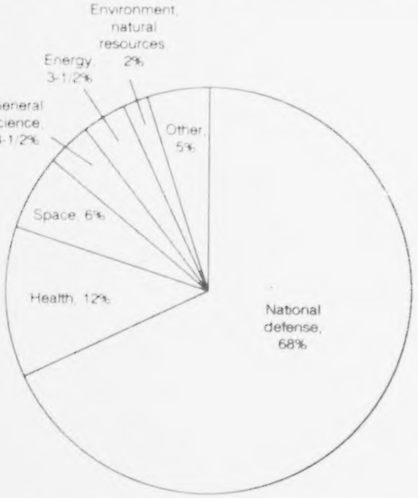
	Obligational authority (billions of dollars)	Percentage of total Federal R&D budget
Department of Defense (military functions only)	$37.9[b]	63.2%
Department of Health and Human Services	7.2	12.0
Department of Energy	5.1	8.5
National Aeronautics and Space Administration	4.8	8.0
National Science Foundation	1.5	2.5
All others	3.5	5.8
	$60.0	100%

[a] Excludes $2 billion in obligations for R&D facilities.

[b] The three services expect to commit a total of $29.4 billion in fiscal 1988—$15.2 billion for the Air Force, $9.5 billion for the Navy, and $4.7 billion for the Army. Adding in the rest of the DoD R&D budget (e.g., spending by agencies such as the Defense Advanced Research Projects Agency) brings the total to $37.9 billion.

SOURCE: *Special Analyses, Budget of the United States Government, Fiscal Year 1988* (Washington, DC: U.S. Government Printing Office, 1988), pp. J-3, J-5.

Figure 1.—U.S. Government R&D by Mission, 1988



SOURCE: *Federal R&D Funding by Budget Function, Fiscal Years 1987-1989*, NSF 88-315 (Washington, DC: National Science Foundation, 1988), p. 4.

Superconducting Super Collider will lead to results of much practical use in the foreseeable future. Understanding is the motive.

Other projects, likewise defined as basic research for budgetary purposes, nonetheless bear quite directly on agency missions. Almost all the R&D funded by the National Institutes of Health (NIH, part of the Department of Health and Human Services) could be termed directed research. NIH supports much fundamental science—e.g., in molecular biology—but it does so with a view toward eventual improvements in health care; many NIH-sponsored projects have quite specific objectives such as a cure for AIDS, or better understanding of the growth of cancerous cells.

Likewise, DoD and DOE R&D serve agency missions. Research in physics laid the foundations for nuclear weapons, with DOE inheriting much of the ongoing support for physics from the Atomic Energy Commission. When the armed services or the Defense Advanced Research Projects Agency (DARPA) sponsor work in the behavioral sciences, they seek insights into the responses of fighter pilots to sensory overloads, or knowledge that will help make artificial intelligence a practical tool for battle management.

Research carried on in industrial laboratories, almost by definition, has a practical orientation. So does engineering research in universities and nonprofit laboratories. Plainly, distinctions such as that between untargeted and directed research will always be arbitrary. Nonetheless, such distinctions help in thinking about R&D and how it supports commercialization.

Within directed research, further distinctions can be made. Incremental work, for example, takes a step-by-step approach toward reasonably well-defined goals. The problems may be technically difficult, but the territory has been at least partially explored. Much of the work on synthesis of new materials that laid the goundwork for the discovery of HTS (box C) falls in this category, as does the many years of R&D aimed at improving the properties of LTS materials.

Box C.—The Discovery of High-Temperature Superconductivity

Two scientists at IBM's Zurich laboratories first discovered rare-earth ceramics with 35-40 °K transition temperatures, announcing their results in 1986.[1] The discovery of superconductors with transition temperatures in the range of 95 °K followed quickly, in February 1987, with early reports coming from Paul Chu's laboratory at the University of Houston. With these discoveries, a race began—a race that is partly a matter of science, and partly a race toward commercialization. The first of the prizes—Nobel laureates for Müller and Bednorz—have already been awarded. Later prizes will take the form of sales revenues and market shares.

The initial work by Müller and Bednorz depended on previous studies carried out in France. In the early 1980s, scientists at the University of Caen, with support from the French Government, first synthesized the barium-lanthanum-copper oxide family of ceramics, reporting their structures and a wide range of property data. The French scientists were engaged in characterizing new classes of materials. Although superconductivity in perovskite ceramics had been found as early as 1975 by scientists at Du Pont, transition temperatures had been low. The French workers did not look for superconductivity in their new perovskite compositions.

The early prominence of the Europeans in HTS research, with later confirmation and extension in the United States and Japan, has many precedents in the synthesis and characterization of novel materials. Indeed, American universities and research laboratories—along with government funding agencies—have neglected the synthesis of new materials.[3]

As for most basic research, relatively well-defined objectives lay behind this work. The French scientists had been synthesizing new perovskite ceramics with the hope of uncovering properties that might ultimately prove useful (e.g., for sensors or catalysis). Practical objectives also motivated the research by Müller and Bednorz in Zurich. No one could have predicted the startling increases in superconducting transition temperatures they found, but higher transition temperatures had been a grail for 75 years. Indeed, Kamerlingh-Onnes, who discovered superconductivity in 1911, quickly mapped out many of the applications that HTS may finally bring to fruition.

Industry, American or foreign, seldom supports the kind of research that led to the new HTS materials. IBM is an anomaly. But even more than these discoveries, the underlying work on synthesis of perovskite ceramics exemplifies the kind of gaps that exist in U.S. R&D—gaps that neither industry nor government has been filling (ch. 4).

In the past, American scientists and American companies were frequently able to take advantage of European R&D on the synthesis of novel materials. In the language of innovation, rather than that of science, the Europeans were "first movers." They created new and unusual materials. Then the Americans, playing the role of "fast second," identified and characterized many of the properties, making important follow-on discoveries. The pattern is not static. The Europeans have begun reorienting their own research programs, seeking to do a better job at later stages in the R&D process (as yet without a great deal of success). The Japanese, already good in these later stages, are striving to become even better, while also moving more heavily into basic research (ch. 3).

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[1] For a review and chronology, see K.A. Müller and J.G. Bednorz, "The Discovery of a Class of High-Temperature Superconductors," *Science*, Sept. 4, 1987, p. 1133. The 35 + ° K results were confirmed first in Japan, and shortly thereafter in the United States and elsewhere. For a Japanese perspective, see S. Tanaka, "Research on High-T_c Superconductivity in Japan," *Physics Today*, December 1987, p. 53. On the French work mentioned in the next paragraph, see C. Michel, L. Er-Rakho, and B. Raveau, "The Oxygen Defect Perovskite $BaLa_4Cu_5O_{13.4}$, A Metallic Conductor," *Materials Research Bulletin*, vol. 20 (1985), p. 667.

Also see app. B, at the end of this report.

[3] F.J. Di Salvo, "New and Artificially Structured Electronic and Magnetic Materials," *Advancing Materials Research*, P.A. Psaras and H.D. Langford (eds.) (Washington, DC: National Academy Press, 1987), p. 161. See especially the table Di Salvo presents on p. 163, which shows that only 6 of 19 new materials in which significant new phenomena have been found (e.g., conducting polymers) were first synthesized in the United States; two reports came from Japan, the rest from Europe. (The paper was written before the first announcement of HTS.) Di Salvo suggests that difficulty in forming and maintaining multidisciplinary research teams in American universities, low funding for materials synthesis from Federal agencies, and low levels of support by corporations account for the European lead in discovering new classes of potentially useful materials.

Most research serves the needs of government or industry. Military needs, social objectives such as health care, and industrial competition have driven the scientific enterprise at least since the end of the 19th century.

COMMERCIALIZATION

Both industry and government support directed research. Promising results lead naturally into development. Research and development then go on in parallel, with research outcomes suggesting new avenues for development, and problems encountered in development defining new research problems.

While the U.S. Government has a long tradition of support for basic research and mission-oriented R&D, it usually leaves pursuit of commercial technologies to the private sector. This policy worked well for many years. For instance, continued development of fiber-reinforced composite materials—lighter and with greater stiffness, strength, and toughness than many metals—builds on a technology base that has been expanding at a rapid rate since the 1950s.[9] The primary stimulus came from the military, where composites found their first applications in missiles, later in manned aircraft. Penetration into commercial aircraft followed.

When it comes to technologies where Federal agencies have been less active, U.S. firms have often fallen behind. Although the U.S. Government has spent several hundred million dollars for R&D on structural ceramics since the early 1970s (app. 2A, at the end of the chapter), the effort has been a small one compared with fiber composites. Japan, meanwhile, has established a useful lead in structural (as well as electronic) ceramics. In semiconductors, American firms established a commanding lead during the 1950s and 1960s, when military procurement provided much of the demand (ch. 4). In later years, as production swung towards civilian markets, Japanese firms closed the gap.

Four Examples

In addition to summarizing Federal programs on ceramics, appendix 2A outlines the evolution of the video-cassette recorder (VCR)—a quite different case from any of those mentioned so far. The appendix also reviews the development of magnetic resonance imaging (MRI) systems, a relatively new product of the medical equipment industry, and LTS magnets. Magnets wound with niobium-titanium alloy—the most widely used LTS conductor—find uses not only in MRI, but in scientific research.

The examples in appendix 2A illustrate something of the range and complexity of commercialization. Sometimes government R&D support is critical (LTS magnets), sometimes nearly irrelevant (the VCR—although much of the underlying technology of magnetic recording did benefit from ongoing government-sponsored R&D). Sometimes governments try to push a technology, to little avail (ceramics for gas turbine engines). For MRI, the major policy impacts had little to do with R&D: commercialization depended on regulatory approvals, as well as Medicare and Medicaid payment policies.

The starting point may be new science, creating new opportunities (MRI, LTS magnets), or it may be the prospect of a huge market if development problems can be solved (VCRs). Inter-firm competition may be intense and international (MRI, VCRs), or it may be largely irrelevant (LTS magnets, where much of the work was undertaken within Federal laboratories).

Government agencies supported R&D on LTS magnets as part of larger, ongoing programs: high-energy physics research, nuclear fusion. Development of niobium-titanium wire for these magnets has been mostly a matter of painstak-

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[9] *Advanced Materials by Design: New Structural Materials Technologies* (Washington, DC: Office of Technology Assessment, June 1988).

Photo credit: University of Kansas Medical Center

Magnetic resonance image of human face

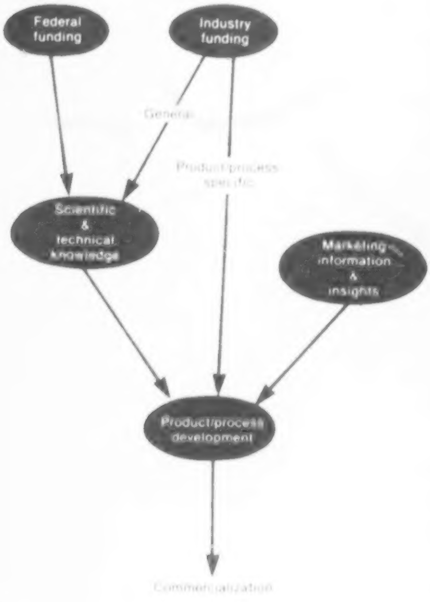
ing engineering. Federal funds paid for much of the work.[10]

The U.S. Government pushed structural ceramics technologies for different reasons. Most recently, DOE has supported work on ceramics for gas turbine engines, hoping to overcome their efficiency limitations; with greater fuel economy (and low enough manufacturing costs), the hope was that turbines could compete with gasoline and diesel engines for automotive applications. While these objectives are consistent with DOE's mission, there has been little pull from the marketplace.

Inputs to Commercialization: Technology and the Marketplace

Product or process development—whether adapting LTS magnet technology for medical imaging systems, or generic techniques for computerized process control to steelmaking—depends on at least two inputs from outside the development group itself. The first of these is knowledge drawn from the technology base, including science, engineering, and shopfloor know-how (figure 2). The second input is knowledge of markets—what potential customers want and need. Steelmakers may improve their process control systems because their customers want better formability, which requires more precise control of melt chemistry. The purchasers of steel may be seeking to provide

Figure 2.—The Process of Commercialization


Federal funding
Industry funding
General
Product/process specific
Scientific & technical knowledge
Marketing information & insights
Product/process development
Commercialization

SOURCE: Office of Technology Assessment, 1986

their own customers with products (automobile fenders, dishwashers) that have better-looking painted surfaces.

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[10] "Superconductive Energy Storage," vol. IV, DOE/ET/26602-35, Final Technical Report, January 1976 to October 1981, prepared by the Applied Superconductivity Center, University of Wisconsin-Madison, for the U.S. Department of Energy, July 1983, ch. III.

R&D and Marketing

Innovations follow their own paths. Figure 3 summarizes the later stages for MRI—those after initial research and feasibility demonstration. Science came first, the complete chronology beginning in 1936 with theoretical predictions of the underlying phenomenon of nuclear magnetic resonance. Experimental demonstrations followed a decade later, with the first two-dimensional images (e.g., of a wrist) in 1973.

Heavy continuing involvement by physicians and scientists made MRI something of an exception. Normally, commercialization is a job for engineers, supported on the one side by knowledge flowing from the technology and science base, and on the other by information on customer, wants, needs, and perceptions.

Much of the early work in HTS will be undertaken by multidisciplinary groups including physicists, chemists, materials scientists, and ceramists, along with electrical, chemical, and mechanical engineers. The known HTS materials are oxide ceramics—brittle and difficult to work with. Learning to use them means drawing where possible on past R&D—work undertaken earlier and for other purposes on structural and electronic ceramics, as well as processing, fabrication, and design techniques from microelectronics.

As applications come into view, companies will call on marketing tools ranging from feasibility studies (which may include detailed projections of manufacturing costs) to consumer surveys. Technical objectives shift as prospective markets emerge; some firms use "technology gatekeepers" to help match research results and market needs. This is an area where U.S. and Japanese strategies in HTS contrast markedly, with Japanese companies much quicker to begin thinking about applications and the marketplace (see ch. 3).

Judging market prospects can be harder than judging prospects for technical progress. Furthermore, market prospects often depend on technological capabilities. Early efforts by Matsushita and Toshiba to design VCRs for household use failed: production costs were high; recording times were short. Not many people would pay upwards of $1000 for a machine limited to 30 minutes per cassette. But improvement was steady. RCA's VideoDisc died in the marketplace in part because the company miscalculated the speed with which VCR manufacturers could reduce their costs to match RCA's target price (initially, $500 at retail). RCA also underestimated the weight consumers would place on off-the-air recording capability, and, failing to grasp the implications of rapidly growing rentals of videotapes, prohibited rentals of its discs.

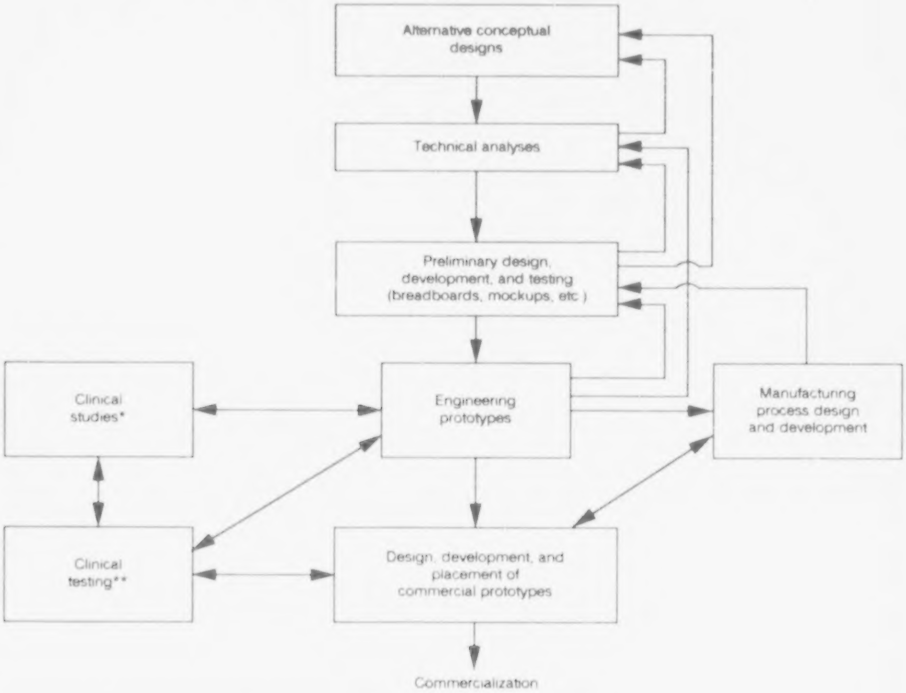
HTS Markets

It is too early to reach many conclusions about markets for HTS. The more obvious high-current, high-field applications—magnets, electric generators, coil and rail guns for the military—have all been analyzed for feasibil-

Photo credit: Argonne National Laboratory

HTS wire, flexible before firing

Figure 3.—Development Stages for Magnetic Resonance Imaging (MRI) Systems


Alternative conceptual designs
Technical analyses
Preliminary design, development, and testing (breadboards, mockups, etc.)
Clinical studies*
Engineering prototypes
Manufacturing process design and development
Clinical testing**
Design, development, and placement of commercial prototypes
Commercialization

*Primarily aimed at developing the technology of MRI and learning to use it (establishing efficacy, subject safety, etc.)
**Primarily aimed at testing prototype MRI system

SOURCE: Based on *Health Technology Case Study 27: Nuclear Magnetic Resonance Imaging Technology* (Washington, DC: Office of Technology Assessment, September 1984), ch. 4

ity, but no one knows much about making practical wire, cables, or current-carrying tapes from the new materials (app. B). These will need higher current densities than yet in view.

Good thin film fabrication methods, the precondition for applications to sensors and electronics, will probably be easier to achieve. Even so, as of mid-1988, there had been no public announcements of reproducible HTS Josephson junctions (JJs). Many of the technical questions on which practical applications depend will not be answered until R&D groups learn to fabricate JJs easily.

Later sections of the report discuss these technical matters in more detail. Here the point is simply that, until the technological prospects come into sharper focus, it will be impossible to do more than speculate about markets. And even then, uncertainty will remain high. No one—scientists, engineers, marketing specialists, science fiction writers—can predict with much accuracy how a new technology like this will eventually be applied. Nor can potential customers say what they might want, or be willing to pay, if they cannot imagine the possibilities. It is the unexpected that will probably have the greatest impact.

Success and Failure

What makes for success or failure in the marketplace? Product and process engineering, marketing skills, luck, sometimes research results. No one has a recipe, any more than a recipe for room-temperature superconductivity.

Costs are central for some products, but for others — MRI is one — competition revolves around non-price features. Many hospitals will readily pay a premium of several hundred thousand dollars for an MRI system with superior imaging performance. At the same time, small private clinics or rural hospitals make up a niche market for which a number of manufacturers have designed low-cost systems.

Products may come out too late or too early. A company may fall behind its competitors and never get much market penetration. Early innovators in the semiconductor industry have sometimes failed and sometimes succeeded.[11] The pioneer minicomputer manufacturer, Digital Equipment Corp., whose PDP-8 established this part of the market, went on to become the second largest computer firm in the world. On the other hand, the microcomputer pioneers—Altair, Imsai, Polymorphic Systems—disappeared. Toshiba invented helical scan recording but the company ended up licensing Sony's Betamax technology (which itself has lost much ground to VHS).

Cost and Risk

As firms move further along the development path, mistakes become more costly. Only one of ten projects launched at the R&D stage ever brings in profits. Before reaching the marketplace, half of all R&D projects fail for technical reasons; poor management or financial stringencies kill two or three more. Of those that do enter production, some never earn enough to cover development costs.

The vast majority of project budgets go for product engineering, process design and development, tooling and production start-up, and test marketing. Introducing an MRI system means investments of $15 million and up for R&D alone; pilot production and field trials require much larger financial commitments. Seldom does research account for more than 10 percent of total project outlays, although the distribution of costs varies a good deal from project to project and industry to industry. The distribution also varies between the United States and Japan.

As table 2 shows, Japanese companies (for the industries and time period examined) spent a bit less on R&D than the average American firm, and much less on manufacturing startup and product introduction. They budgeted more in gearing up for production—on facilities, tooling, and special manufacturing equipment (a difference that may also reflect higher projected volumes). Japanese firms no doubt have lower startup costs because they invest more in front-end process development. Yet a substantial difference remains. Adding the percentages for tooling and equipment to those for manufacturing startup gives a total of 40 percent for the U.S. companies, 54 percent for the Japanese. The greater proportion of total project expenses for tooling and equipment reflects the higher priorities Japanese managers place on manufacturing as an element in competitive strategy.

Such priorities will make a difference in commercialization of HTS, which will depend critically on process know-how. U.S. firms have underinvested in process technology for years—one reason for competitive slippage in industries ranging from steel to automobiles to electronics.

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[11] See, for example, *International Competitiveness in Electronics* (Washington, DC: Office of Technology Assessment, November 1983), "Appendix C: Case Studies in the Development and Marketing of Electronics Products, Semiconductors: The 4K Dynamic MOS RAM," pp. 524-531.

Table 2.—Distribution of Costs for Development and Introduction of New Products and Processes[a]

	Percentage of total project cost				
	Research, development and design	Protoype or pilot plant[b]	Tooling and equipment	Manufacturing startup	Marketing startup
U.S. companies	26%	17%	23%	17%	17%
Japanese companies	21	16	44	10	8

[a] Survey figures from 1985 for 50 matched pairs of U.S. and Japanese firms. The total of 100 included 36 chemical companies, 30 machinery, 20 electrical and electronics, and 14 from the rubber and metals industries.
[b] For cases of product development, the costs are for prototyping; for process development, they include investments in pilot plants.
NOTE: Totals may not add because of rounding.

SOURCE: E. Mansfield, "The Process of Industrial Innovation in the United States and Japan: An Empirical Study," unpublished seminar paper presented Mar. 1, 1988, in Washington, DC.

Competitive Advantage

What does it take to use technology effectively? The examples mentioned above, and others, point to the following common factors:

- *Appropriate use of technology and science*, new and old—whether a company generates the knowledge internally, or gets it elsewhere. Much of the science base for HTS will be available to everyone. To establish a competitive advantage, companies will have to develop proprietary know-how, and do it ahead of their competitors.
- *Effective linkages between engineering and marketing*. Customers for many of the early applications of HTS—in military systems, electronics, or perhaps energy storage—will be technically astute. Marketing will count, but not so heavily as for consumer products.
- *Effective linkages between product development groups and manufacturing*—a point already stressed for HTS.
- *Managerial commitment to risky and uncertain R&D projects*. The next chapter explores this dimension more fully for HTS.

What are the conditions under which American firms have trouble in commercialization—in the *effective utilization of technical knowledge*, new or old? Under what circumstances do American firms perform best? Effective policies depend on the answers to such questions.

Generally speaking, OTA assessments have found the problems to be most acute when it comes to applications of *existing technology* by firms in *older industries*—and particularly when it comes to shopfloor *manufacturing technologies*. In the earlier years of high technology, the United States had potent competitive advantages: entrepreneurship and venture capital; a decentralized science infrastructure with many centers of excellence both inside and outside the Nation's universities; flexible labor markets, with high mobility among engineers, scientists, and managers. These strengths have begun to wane. In many industries, Japanese companies are out-engineering American firms. Even in high technology, the Japanese have been able to move quickly from the laboratory to the marketplace. The days when U.S. companies could take their time in commercializing R&D are past.

STRENGTHS, WEAKNESSES, AND STRATEGY

What does the discussion above (and in app. 2A) imply for U.S. abilities in commercialization? The first point is simply that taking a new product into the marketplace is always difficult. In their efforts to penetrate the U.S. market, Japanese automakers suffered from many problems they failed to anticipate. Powertrains wore out quickly in long-distance driving. Companies like Honda found themselves trying to sell cars with fenders that would rust through after one or two northern winters. Federally mandated recalls were frequent.

Product/Process Strategies

Japan's automakers overcame these difficulties. They redesigned their products to suit U.S. needs and tastes, establishing deserved reputations for quality and reliability. They built strong dealer organizations that helped them understand American consumers. In contrast to European manufacturers, the Japanese began developing vehicles specially tailored to the U.S. market—small pickup trucks, four-wheel drive vehicles, sports and luxury models. Most were variants on products sold in Japan and other foreign markets, but a few—such as Nissan's Pulsar—were designed primarily in the United States to appeal to Americans.[12]

Japan's automakers learned many lessons from their American rivals, and learned them well. The credit goes to the industry, which benefited from government policies, but not nearly so directly as, say, Japanese computer manufacturers. In the past several years, with their upmarket moves and new brand names, Japan's automakers have taken another leaf from Alfred Sloan: in turning their automobiles into high-fashion products, they have introduced new models much more quickly than American or European firms—a necessary capability for implementing such a strategy.

Design/development/tooling cycles for Japanese automakers have shrunk to little more than half those in the United States; Honda's model cycle is down to 40 months, while American firms take 5 or 6 years.[13] U.S. automakers have looked to computer-aided engineering to narrow the gap. The Japanese, however, appear to succeed through quite conventional approaches to engineering development, carefully managed. Certainly they do not have the lead in such computer-intensive techniques as numerical analysis of vehicle structures, aerodynamic modeling and simulation, or analytical predictions of vehicle ride, vibration, and handling.

The high-fashion, product differentiation strategy is new for Japanese companies only in the automobile industry. It is one the Japanese have used in the past in cases like consumer electronics and motorcycles. Successful targeting of markets—whether for consumer goods, for capital equipment (machine tools), or for intermediate products (semiconductor chips)—has been a hallmark of Japan's competitive success.[14]

As discussed in the next chapter, Japanese companies have already put a good deal of effort into thinking about new applications of superconductivity; they may well locate some of the possible market niches before American firms. The Japanese have often carved out substantial markets by starting from small niches; large integrated Japanese firms have been more

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[11] J. Yamaguchi, "Quick-change open-top car matches closed coupe in body rigidity," *Automotive Engineering*, February 1987, p. 167.

When Toyota models got poor ratings on U.S. crash tests, the company quickly made design changes that upped their scores—L. McGinley, "Car Crash Rankings: Safety Guide Or Numbers That Don't Add Up?" *Wall Street Journal*, Dec. 1, 1987, p. 39.

[13] "Honda's R&D Mastermind," *Automotive Industries*, November 1987, p. 52. More generally, see H. Takeuchi and I. Nonaka, "The new new product development game," *Harvard Business Review*, January-February 1986, p. 137; J. Bussey and D.R. Sease, "Manufacturers Strive To Slice Time Needed To Develop Products," *Wall Street Journal*, Feb. 23, 1988, p. 1; R. Poe "American Automobile Makers Bet On CIM To Defend Against Japanese Inroads," *Datamation*, Mar. 1, 1988, p. 43; K.B. Clark and T. Fujimoto, "Overlapping Problem Solving in Product Development," working paper, Harvard Business School, April 1988. Part of the Japanese advantage may come simply from putting more engineers to work: GM, Ford, and Chrysler employ a total of 30,000 engineers; Toyota, Honda, and Nissan more than 40,000—J. McElroy, "Outsourcing: The Double-Edged Sword," *Automotive Industries*, March 1988, p. 46.

While it takes much longer for American firms to introduce new products in some industries, according to a recent survey the U.S.-Japan difference in design and development times does not hold across the board—E. Mansfield, "The Process of Industrial Innovation in the United States and Japan: An Empirical Study," unpublished seminar paper presented Mar. 1, 1988 in Washington, DC. Professor Mansfield's survey does show that Japanese companies were generally much quicker than American firms when product development efforts began with licensed technologies. Moreover, Japanese firms willingly absorb substantially higher costs to shorten their development cycles.

[14] On the Japanese approach to product planning and marketing, see J.K. Johansson and I. Nonaka, "Market research the Japanese way," *Harvard Business Review*, May-June 1987, p. 16; P. Marsh, "The ideas engine which drives Japan," *Financial Times*, May 29, 1987, p. 14; P. Marsh, "Why research is in the driving seat," *Financial Times*, June 2, 1987, p. 12; C. Lorenz, "'Scrum and Scramble'—the Japanese Style," *Financial Times*, June 19, 1987, p. 19; P.S. Leven, "Repatriate Product Design," *Across the Board*, December 1987, p. 39; C. Rapoport, "How Honda research runs free and easy," *Financial Times*, Feb. 16, 1988, p. 10.

aggressive than their American counterparts in pursuing specialized products, including advanced materials. *Japanese companies are willing to start with small-volume production and grow with their markets—a strategy likely to prove successful in HTS, indeed one that may prove necessary.*

How do companies based in Japan do so well at defining and attacking market segments, particularly in countries foreign to them? Most Japanese companies do use market research techniques, although table 2 showed they spend less on this than American companies. As some U.S. firms also realize, the best marketing research often remains as informal today as it was 50 years ago—a matter of good judgment from within the company more than consulting firms, focus groups, and consumer surveys.

Japanese firms in many industries have also capitalized on the quality of their goods. Lagging quality not only leaves customers unhappy, it raises manufacturing costs. Quality and reliability problems have plagued American industries ranging from automobiles to semiconductors. Careful control of the production process will be necessary for fabricating the new HTS materials, as it is for high-technology electronic and structural ceramics, or for integrated circuits.

The primary point is this: by the 1960s, American firms had come to think of their skills in engineering and marketing as far and away the best in the world. If this was true then, it is true no longer. Many U.S. companies have not yet faced up to the need to do better. Others periodically rediscover such well-known management and engineering practices as simultaneous engineering, design for production, or quality engineering, but fail to follow through with actions that institutionalize them. Some still look to techniques like quality circles for miracle cures.

Research, Development, and Engineering: Parallel or Sequential?

Simultaneous engineering means nothing more than tackling product and process development in parallel, with overlapping responsibilities in design and manufacturing groups, if not a fully integrated approach. Simultaneous engineering may be hard to achieve in a modern American corporation, but in principle is nothing but common sense. A hundred years ago, technology was simpler and no one had discovered any need to separate design and manufacturing.

The chain can be extended back to research. But given the uncertainties that accompany the search for new knowledge, and the high costs of downstream development, many U.S. executives have come to view research, development, and product planning as sequential processes. Only when consistent, verifiable, and potentially useful results begin to emerge from the laboratory do American companies think about incorporating engineers into the effort. Even at this point, research may remain separated from development: the scientists pass along their findings, but the two groups continue to work independently. Under these circumstances, the entire process can become almost purely sequential—running from applied research to product planning and development to manufacturing engineering, with little overlap.

Technology-based Japanese companies, in contrast, have developed simultaneous or parallel processes to a high level. Many are now busy integrating backward into research—a task they see as necessary for commercializing high technologies like HTS. Already, they do a better job of responding to design and marketing requirements through incremental, applied research.

Japanese managers, moreover, tend to be optimistic about research in general and about HTS specifically (ch. 3). Perhaps because they mix development and engineering personnel into project groups at an early stage, the belief seems pervasive that useful results of one sort or another will inevitably emerge from HTS R&D. Japanese managers have strong convictions on these matters. They believe it wrong to think about technical developments as proceeding more-or-less linearly from basic research to applied research, then to development and product design, and finally to process engineering. More to the point, they are acting on these beliefs in HTS.

American managers know just as well that many of the steps should take place in parallel. But for reasons ranging from trouble in learning to manage parallel processes effectively (one reason for longer product development cycles), to the characteristics of U.S. financial markets, they do not always act on this knowledge. When it comes to HTS, American managers have been relatively cautious; they want to see results from the laboratory before taking the next step.

COMMERCIALIZING HTS

There is a bright side. The United States retains major sources of strength in commercializing new technologies. Table 3—which draws heavily on past OTA assessments of competitiveness—summarizes advantages and disadvantages of U.S. firms. Table 4 outlines the implications for HTS. Later chapters expand on many of the points in these two tables, particularly where the Federal Government has policy leverage.

Table 3 has a simple message: the United States has a number of areas of advantage, coupled with several serious handicaps. Those handicaps—emphasis on short-term financial paybacks, low priorities for commercial technology development and for manufacturing—have put U.S. firms at a severe disadvantage in competing with Japan. Some of the consequences can already be seen in HTS.

On the other hand, American firms have often been successful—at least in the past—when new science has led to new products and new industries, especially where fast-growing and volatile markets promise rich rewards (table 3, factor 1). American companies perform less well, and often poorly, at incremental innovation—more-or-less routine improvements to existing products and processes. These kinds of problems have been much more prevalent in steel than in chemicals, in machine tools than in computer software, in automobiles than in commercial aircraft.

Most of the success stories came in the years before U.S. industry had much to worry about from international competition. Table 4 summarizes the lessons that past performance and events thus far hold for HTS, and compares the strengths and weaknesses of American companies with those in Japan. Some of the U.S. entrants will be new companies, started specifically to exploit HTS and staffed by people with strong credentials in related fields of science and technology. Other firms will move in from a base in LTS. Both kinds of companies should be able to respond effectively to the problems and opportunities that emerge in the early years of HTS—with good ideas and a strong science base, together with venture capital and entrepreneurial drive, leading to success in specialized products and niche markets.

The picture could change as the technology stabilizes and financial strength becomes more important. When production volumes increase, manufacturing capabilities will grow more important. Companies will have to carefully tailor products to emerging markets, and find capital for expansion. U.S. industries that flourished as infants have run into difficulty as competitors—primarily the Japanese—caught up and pulled ahead in the race to capitalize on new approaches to factory production or new knowledge concerning electron devices; in the years ahead, the biotechnology industry could stumble, just like the semiconductor industry.[15]

Microelectronics, and Other Precedents

A decade ago the semiconductor industry still seemed a bastion of U.S. strength. The Japanese were nibbling at the margins, no more. Today, the Federal Government finds itself putting money into the new consortium Sematech, trying to help American firms regain a technological lead lost seemingly overnight.

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[15] So far, however, there has been little sign of such slippage. See *New Developments in Biotechnology: U.S. Investment in Biotechnology* (Washington, DC: Office of Technology Assessment, July 1988).

Table 3.—U.S. Strengths and Weaknesses in Commercialization

U.S. strengths	U.S. weaknesses	Comments
Factor 1. Industry and market structure; market dynamics.		
In the past, U.S. firms performed well in rapidly growing industries and markets, especially during the early stages in R&D-intensive industries.	American companies have had trouble coping with slow growth or contraction. Although new technologies promising greater productivity might improve competitive performance in industries like steel, corporate executives frequently choose to invest in unrelated businesses. Where foreign firms might take a more active approach to managing contraction, American companies sometimes let troubled divisions struggle along, without new investment, until profits disappear. Then they shut the doors.	Other countries frequently look to public policies to help companies and their employees adjust to decline.
Factor 2. Blue and gray collar labor force.		
High labor mobility helps American companies attract the people they need.	Many development projects depend on craftsmen who can fabricate prototypes and modify them quickly based on test results and field experience. In some U.S. industries shortages of skilled labor—e.g., technicians, modelmakers—have begun to slow commercialization. U.S. apprenticeship programs have been in decline. Vocational training reaches greater fractions of the labor force in nations like West Germany; large Japanese companies invest more heavily in job-related training for blue- and gray-collar employees than do American firms.	In the past, U.S. wage rates worked to the disadvantage of American firms, while creating incentives for investments in R&D and new manufacturing technologies that could raise productivity. Today, international differences in labor costs are less of a factor than in the 1970s.
Factor 3. Professional and managerial work force.		
Mobility among managers and technical professionals has stimulated early commercialization in high-technology industries. New products have reached the marketplace more quickly because people have left one company and started another to pursue their own ideas. Deep and well-integrated financial markets—e.g., for venture capital—have helped.	American companies underinvest in process (as opposed to product) technologies. This is part of a bigger problem: too many managers and engineers in the United States avoid the factory floor: • for managers, marketing or finance has been the road to the top; • engineers—schooled according to an applied science model—have been insensitive, not only to role of manufacturing, but to the significance of design and marketing. Put simply, the engineering profession has divorced itself from the marketplace, and the needs and desires of potential customers (particularly when it comes to consumer products). Compounding these problems, many American companies underutilize their engineers. Finally, many U.S. firms provide little support for continuing education of their technical employees. Managers and professionals in the United States sometimes place individual ambition over company goals. Competition among individuals may make cooperation within the organization more difficult (e.g., between product engineering and manufacturing).	More upper level managers in Japanese and West German firms have technical backgrounds than in the United States; they appear more sensitive to the strategic significance of manufacturing, and in at least some cases to new technological opportunities.
Factor 4. Industrial infrastructure (also see Factor 6 below).		
American companies can call on a vast array of vendors, suppliers, subcontractors, and service firms for needs ranging from fabrication of prototypes to financing, legal services, and marketing research. Few other countries have a comparable range of capabilities so easily available.[8]	U.S. competitiveness in capital goods like machine tools has slipped, compounding the problems in manufacturing technology. Arms-length relationships between American firms and their vendors and suppliers may not be as conducive to commercialization as the relationships found in Japan (relations which might be classified as close and cooperative, or perhaps with equal accuracy as coercive and dependent).	At present, the independent computer software and services industry is perhaps the preeminent illustration of U.S. infrastructural strength.

[8] On the importance of specialty firms for the U.S. microelectronics industry, particularly those supplying semiconductor manufacturing equipment, see *International Competitiveness in Electronics* (Washington, DC: November 1983), pp. 144-145. On service inputs, see *International Competition in Services* (Washington, DC: July 1987), pp. 32-34 and 55-57.

Table 3.—U.S. Strengths and Weaknesses in Commercialization—Continued

U.S. strengths	U.S. weaknesses	Comments
Factor 5. Technology and science base (also see Factor 7 below)		
U.S. strength in basic research—both science and engineering—has been a cornerstone of commercialization. The national laboratory system is a major resource, although one that has not been turned to the needs of industry. Multidisciplinary R&D—essential in industrial (and government) laboratories—has been the exception rather than the rule in American universities. Foreign university systems, however, have probably been even worse at multidisciplinary research.	U.S. strength in basic research has not always been matched by strength in applied research, nor in the application of technical knowledge. The Nation depends heavily on a relatively small number of large corporations for industrial R&D and the development of new commercial technologies. When R&D is not close enough to anyone's interests, gaps open in the technology base. Moreover, U.S. firms seem to be falling behind in their ability to move swiftly from the R&D laboratory to the marketplace. Diffusion of technology within the U.S. economy has been a persistent and serious problem.	The science base and technology base are not identical. The latter spreads much more broadly, encompassing, for instance, the intuitive rules and methods—many of them tacit rather than formally codified—that lie at the heart of technological practice. The semiconductor and biotechnology industries have both sprung from scientific advances. But the theoretical foundations for each remain relatively weak. As a result, progress depends heavily on experience and empirical know-how—again, part of the technology base but not the science base.
	American engineers and their employers have often remained unfamiliar with technologies developed elsewhere, reluctant to adopt them. This reluctance is evident, for instance, when it comes to rules of thumb and informal procedures—sanctioned by experience if not by scientific knowledge. Examples include shop-floor practices for job scheduling and quality control.	Japanese and German firms give commercial technology development higher priorities. Governments in these countries also give more consistent support to generic, pre-competitive R&D.
Factor 6. The business environment for innovation and technology diffusion (also see Factor 7 below)		
Clusters of knowledge and skills such as found in the Boston area, or Silicon Valley, help speed commercialization. While some of this entrepreneurial vitality can be linked to major research universities, other regions have become centers of high-technology development even though lacking well-known schools like MIT or Stanford.	Many American firms seem preoccupied with home runs—major breakthroughs in the marketplace—unwilling to begin with niche products and grow gradually.	Linkages between universities and industry could be stronger, but nonetheless probably function better in the United States than elsewhere.
The size and wealth of the U.S. market, and the sophistication of customers—especially business customers—work to the advantage of innovators; indeed, foreign companies sometimes come to the United States simply to try out new ideas.	Poor labor relations sometimes slow adoption of new technology. Reluctance among American engineers and managers to learn from shop-floor employees hurts productivity and competitiveness. Companies in other parts of the world may be somewhat more willing to cooperate in R&D.	Business and consumer confidence encourage innovation and rapid commercialization. Over the past few years, business confidence appears to have ebbed somewhat—a casualty of Federal budget deficits, trade imbalances, rapid exchange rate swings, and the evident inability of the Government to address these issues. At the same time, the political stability of the United States remains a major strength.
Factor 7. The policy environment for innovation and technology development		
The United States has a deeply rooted commitment to open markets and vigorous competition. (So does Japan, when it comes to domestic markets and domestic competition.) With widespread economic deregulation since the early 1970s—plus a tax system and financial markets that reward entrepreneurs—startups and smaller companies have often been leaders in commercializing new technologies. Purchases by the Federal Government have stimulated some industries, particularly in their early years. Examples range from aircraft and computers to lasers and semiconductors. A broad range of other U.S. policies—e.g., strong legal protections for intellectual property—helps companies stake out and exploit proprietary technological positions.	Deregulated U.S. financial markets bear some of the blame for the risk aversion and short-term decisions common in American business. Sometimes U.S. regulatory policies delay commercialization. Examples include approvals for drugs and pharmaceutical products.	Many government policies act on commercialization indirectly. Industries have evolved in different ways in different countries, in part because of these influences. • Along with antitrust, financial market regulations—e.g., rules covering holdings of stock in one company by others—affect the extent of vertical and horizontal integration. • Tax policies—treatment of capital gains, R&D and investment tax credits—influence corporate decisions on investments in new products and processes. • Antitrust enforcement helps set the environment for inter-firm cooperation in R&D. • Trade protection can reduce the risks of new investment, thereby stimulating commercialization. On the other hand, protected firms may grow complacent and decline to invest in new technologies. • Technical standards sometimes act to speed the adoption of new technologies. If premature or poorly conceived, however, they can impede commercialization. • Education and training have enormous long-run impacts on commercialization and competitiveness.

SOURCE: Office of Technology Assessment, 1988.

As yet, no one knows very much about the technical problems that will have to be overcome in commercializing the new superconductors. Still, parallels have begun to emerge. In microelectronics, product and process know-how are closely tied.[16] This will also be the case in HTS, where the companies that move down learning curves the fastest will reap competitive advantages.

Semiconductor firms must grapple with difficult technical problems in the heat of fierce competitive struggles: understanding the effects of purity and defect population in the silicon crystals with which production begins; process variables for the steps in diffusion or formation of oxide layers. Costs depend on yield—the fraction of functional chips produced. Both yield and quality depend on the design of the chip as well as control of the manufacturing process. With the technology of semiconductor devices ahead of the underlying science, chip designers and process engineers must proceed on a largely empirical basis as they work toward ever denser and more powerful circuits. New applications of HTS will likewise require tailoring of material properties on a microscopic scale, probably without much theoretical guidance.

Companies in the semiconductor industry must solve problems today so they can compete in the marketplace tomorrow. HTS is not

Table 4.—U.S. Advantages and Disadvantages in Commercializing HTS

U.S. advantages	U.S. disadvantages	Comments
Factor 1. Industry and market structure; market dynamics.		
New science and technology make for conditions under which American firms should be able to commercialize quickly and compete effectively.	At some point, financing constraints may make it difficult for startups and smaller U.S. companies to continue in HTS on an independent basis. Mergers may be necessary for growth.	Past U.S. successes in high technology came when international competition was a minor factor. Foreign firms have now proven they can move quickly from the R&D stage to the marketplace. Mergers or other arrangements driven by financing needs sometimes help, sometimes hurt. Ties with larger companies may stifle innovation. In biotechnology, linkages between small firms and larger companies have helped with regulatory approvals and process scale-ups. American semiconductor firms, however, have seldom been willing to sacrifice their independence for new capital—one reason they have fallen behind large, integrated Japanese competitors.
Factor 2. Blue and gray collar labor force.		
Some American companies start with a core of employees having experience in low-temperature superconductivity (LTS). A portion of these skills will translate to HTS. At the same time, given that the new HTS superconductors are fundamentally different materials—ceramics rather than metals—a wide array of quite different skills will be needed. Some of the skilled employees may come from related industries, including electronics.	Japanese companies with ceramics businesses can draw on larger numbers of people with relevant skills. These employees will help give Japan a head start in certain kinds of HTS R&D—e.g., mechanical behavior, processing and fabrication. Japanese firms also have many workers with extensive and transferable experience in microelectronics.	So far, few American ceramics firms have been prominent in HTS R&D.
Factor 3. Professional and managerial work force.		
Managers, engineers, and scientists moving into U.S. HTS companies from industries like microelectronics will bring new insights and new ideas.	Decisionmakers in American companies, large and small, may not be willing or able to make long-term commitments to HTS-related R&D, particularly more basic work.	At least initially, HTS startups will have managerial staffs with strong technical backgrounds. Some larger U.S. firms with the resources to compete in HTS-related markets

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[16] *International Competitiveness in Electronics*, op. cit., ch. 6. As the example of Trilogy Systems illustrates, firms must be able not only to design, but to build new types of devices; Trilogy had to abandon its planned line of computers after finding it could not fabricate the wafer-scale integrated circuits required.

Table 4.—U.S. Advantages and Disadvantages in Commercializing HTS—Continued

U.S. advantages	U.S. disadvantages	Comments
	Processing and fabrication will pose difficult technical problems, of a sort that American companies have not been very good at solving. Much of the R&D needed to develop HTS will be empirically-based engineering, with heavy doses of trial and error. Japanese companies do very well at this kind of development, often better than their American counterparts.	may chose other investments because managers fail to understand the technology or recognize the opportunities. Managers with previous experience in LTS may tend to err on the side of conservatism. On the other hand, HTS has had more than its share of exaggerated publicity already. A cautious view of HTS, born of past experience in LTS, could prove realistic.
Factor 4. Industrial infrastructure (also see Factor 6 below)		
The generally strong U.S. infrastructure for high technology should be an advantage in HTS.	When it comes to the science and technology of ceramics, specifically, the U.S. infrastructure is weak. American HTS companies with ceramics-related technical problems may have trouble finding help.	Japan's HTS infrastructure exists mostly inside large, integrated companies. In the United States, startups will have to rely heavily on help from outside. The U.S. approach has advantages in flexibility and creative problem-solving, while Japan's reliance on internal resources creates reservoirs of skills and expertise that will be very effective over the longer term.
Factor 5. Technology and science base (also see Factor 7 below)		
Despite lack of attention to ceramics compared with Japan, the United States has a relatively strong base in materials R&D. In the early years of HTS development, the defense emphasis of federally supported R&D will work in some ways to the U.S. advantage. Funding from the Department of Defense (DoD) will help train engineers and scientists, and may support the development of some dual-use HTS technologies (e.g., powerful magnets). DoD support for processing R&D could be especially important. A number of national laboratories have the resources, including specialized equipment, to help with the technical problems of HTS.	Military and civilian applications of HTS will diverge rapdily, limiting the spillover effects from DoD R&D spending.	In 1986, U.S. engineering schools granted 3700 PhDs—but only 14 in ceramics. Without major policy shifts, Federal agencies will fund little R&D that directly supports commercialization. Nonetheless, the United States is beginning to address the problems of transferring federally funded R&D to industry. American companies will probably be at a disadvantage for years to come in solving the manufacturing-related problems of HTS. To make progress here, American scientists and engineers—including those engaged in university research—must be willing to spend more of their time working on industrial problems (even if the scientific and university communities continue to view practical work as less than fully respectable). Without substantial efforts in manufacturing R&D, some American companies could be forced into partnerships with Japanese firms simply to get access to processing know-how.
Factor 6. The business environment for innovation and technology diffusion (also see Factor 7 below)		
U.S. markets should prove receptive to new products based on HTS. Some foreign companies could find they need an R&D presence here simply to keep up.	With Japanese firms starting on a par with American companies, know-how from abroad may prove essential for keeping pace. Many American companies have been unable or unwilling to reach useful technology transfer agreements with Japanese firms. Lack of experience in doing business with the Japanese could become a significant handicap in HTS.	University-industry relations in the United States seem to be following patterns similar to those in biotechnology, with strong and productive linkages developing. Small U.S. firms have begun devising strategies for commercializing HTS. Many larger American firms with the resources to compete in HTS, however, seem to be adopting a wait-and-see attitude.
Factor 7. The policy environment for innovation and technology development.		
So far, the U.S. policy approach seems conducive to entrepreneurial startups in HTS. There is little indication that the 1986 changes in U.S. tax law—which increased rates on capital gains—have choked off funds for HTS startups.	After the initial announcement of the Administration's 11-point superconductivity initiative, little was heard for 7 months—a long time in such a fast-moving field. Budgetary uncertainties, moreover, delayed decisions on Federal R&D funding well into the 1988 fiscal year, hampering progress in universities, industry, and the national laboratories.	While Federal procurements helped the U.S. semiconductor industry get off the ground in the 1960s, poor experience with demonstration projects in energy and transportation has soured prospects for some kinds of policy options that otherwise might provide stability and support for HTS during a long period of gestation. Some companies continue to express concern that U.S. antitrust policies will limit opportunities for consortia and other forms of joint R&D. However, OTA has not learned of any case in which U.S. antitrust enforcement has in fact stopped firms from cooperating in R&D.

SOURCE: Office of Technology Assessment, 1988.

yet at this stage. There is no market. The race is still a scientific race. But if HTS lives up to expectations, some of the history of microelectronics may be replayed.

Commercialization, indeed, may begin with specialized electronic devices—perhaps very sensitive detectors of electromagnetic signals, or high-speed digital circuits (app. B). HTS-based devices may be used in conjunction with semiconductors. Other parallels are nontechnical—matters of industrial structure, corporate decisionmaking, and public policy. Relatively small U.S.-based semiconductor firms find themselves competing with vertically integrated Japanese multinationals, enterprises with far more money and manpower. These same Japanese firms have made heavy commitments to HTS R&D. Government policies for HTS in Japan, while far removed from the (false) stereotype of industrial targeting, show many familiar features: notably, pragmatic attention to bottlenecks that might slow commercialization by Japan's very aggressive private sector.

The Japanese firms that have made so much progress with electronic and structural ceramics will be well placed when it comes to fabricating wires, cables, tapes, and other forms of conductors made from the new HTS materials. Learning to make practical conductors from the new materials—for the circuitry inside computers, or for electrical windings in generators or energy storage systems—will require a great deal of trial-and-error development. Japanese companies do well at this kind of engineering. Some of the specific skills in ceramics processing they have developed will transfer, just as will some of their skills in semiconductor processing. American firms, in contrast, have fallen down badly in processing and manufacturing skills over the past two decades.

HTS Technologies

Appendix B outlines prospective applications of HTS (table B-1), including estimated time frames for commercialization (table B-3). Early applications of HTS will be highly specialized—military equipment, niche markets on the commercial side (perhaps in scientific apparatus, or for nondestructive inspection). Japanese firms will provide strong competition from the beginning.

High-Current, High-Field Applications: Electrical Machinery and Equipment

Japan's lack of energy resources means strong motives for commercializing HTS in order to conserve electrical power. Even though superconductivity offers relatively small efficiency increments (because large-scale conventional equipment is highly efficient already), Japanese companies may make more rapid progress than American firms in superconducting motors and generators, as well as transformers and energy storage systems. Similar forces are at work for magnetically levitated trains, where the motivation comes from a heavy existing commitment to fixed-rail passenger transportation—natural in a small and crowded nation like Japan. Summarizing:

- Both the United States and Japan start from a roughly equivalent experience base in LTS motors and generators, but the Japanese have more work underway at present, and will probably pull ahead when and if suitable HTS conductors become available.
- Each country has one or more active LTS energy storage projects (large superconducting rings in which electrical current circulates indefinitely, to be withdrawn when needed). With SDI funding, two U.S. firms are designing a prototype ring that would quickly dump the stored energy into powerful lasers. Japan's R&D has been directed at storage for electric utilities, where discharge rates will be much lower. While the U.S. effort will yield some lessons for utilities, design trade-offs will bias the prototype—and the knowledge gained from it—towards the quick-discharge military application.
- DoD R&D aimed at coil and rail guns and other high-power, high-field applications (e.g., ship propulsion) could strengthen the generic technology base in HTS, helping commercial industries indirectly.
- When it comes to possible applications such as magnetically levitated trains, the

United States starts out behind, having halted R&D in 1975 (see box K, ch. 3). However, it is not yet clear that HTS would offer much advantage here.

- In medical electronics—e.g., MRI—the United States has a substantial lead in know-how and experience, one that should persist (although LTS might again continue to be the technology of choice for some time).

Pursuing most of these applications will demand technical resources and experience, as well as financing, on a scale beyond that of the small, entrepreneurial firms that emerged in the early years of LTS, and those being started today to exploit specialized HTS applications. If big U.S. companies prove reluctant to move into markets for electric power equipment—and smaller entrants cannot—integrated foreign producers will probably take the lead, internationally and perhaps in the U.S. market.

Superconducting Electronics

Progress in thin films for electronics should be more rapid than for the conductors needed in high-power applications. When it comes to electronics, the Japanese will probably benefit to some extent from R&D on Josephson-based computers; government and industry in Japan continued work on JJs for computing after U.S. companies dropped most of their own activity (see box J in the next chapter).

Josephson junctions, however, function only as two-terminal devices, weak amplifiers at best. No one knows how to make useful three-terminal devices like transistors from superconducting materials. A practical three-terminal superconducting device, even one restricted to liquid helium temperatures, could open up a broad range of opportunities. Whether this will be possible is an open question. JJs also make for highly sensitive detectors of infrared and other electromagnetic radiation. LTS sensors—and in the future perhaps HTS sensors (e.g., for satellites, where passive cooling should keep operating temperatures below the transition temperatures of the new materials)—have potentially important military applications. As a result, DoD has funded a good deal of R&D over the years on these devices, as well on superconducting components for very powerful computers. As DoD R&D increasingly focuses on HTS, some of its work—perhaps in sensors—will contribute to commercial spin-offs.

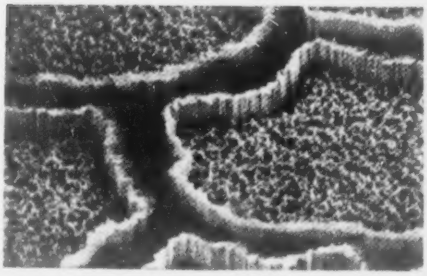

Photo credit: Supercon, Inc.

High magnification image of niobium titanium superconducting filaments

Japanese companies will prove able competitors over the long run in both devices and systems applications of HTS. In their efforts to catch up with IBM and other U.S. computer firms, Japan's integrated manufacturers—several of which make chips, computers, and telecommunications hardware—have been spending heavily on R&D for years. They are seeking a technological window that would help them overtake the United States in high-technology electronics, and particularly in computers—a field where American firms remain broadly superior. The Japanese see HTS as a possible window.

Smaller American firms will probably find electronics markets attractive. Hypres, for example, founded by an ex-IBM physicist after the computer manufacturer scaled back its LTS JJ computer project in 1983, introduced a very high-speed LTS-based data-sampler in 1987. The company hopes its experience base will give it advantages in HTS. Other small LTS specialists also plan to move into HTS by building on their past work with the older materials.

CONCLUDING REMARKS

The next chapter looks in some detail at corporate strategies toward HTS in the United States and Japan. European countries, too, have excellent science and engineering capabilities in both private and public sectors. But longstanding problems in capitalizing on these strengths suggest that European firms will not be able to keep up in the race to commercialize HTS. Box D summarizes the reasons.

Companies everywhere look for proprietary advantages from R&D—patentable inventions, expertise they can protect through trade secrets. Semiconductor companies, for example, each have their own process technology. Much of the information is closely held; some of it is embodied in the skills of their employees. In LTS as well, proprietary know-how helped small companies stake out positions in the manufacture of wire and in specialized electronics.

The Japanese developed a great deal of proprietary technology in commercializing the VCR. The story is one of Japanese success in innovation—engineering design and development, market research, mass production manufacturing. But in related markets like personal computers there is little evidence of slippage, despite many past predictions of a Japanese takeover. Nor have the Japanese been able, for instance, to move from success in high-density memory chips to microprocessors. U.S. leads in computer software, or biotechnology, may have narrowed in the last 5 years, but not by much. Japan's bet on structural ceramics may not pay off; the technical problems of achieving reliability in very brittle materials could prove too difficult.

Scientific knowledge and technological understanding—not the same—interact throughout such development efforts. Sometimes new science leads to new technology. This has been the case in superconductivity, beginning with its discovery in 1911, but especially since the 1960s. In other cases, demand for new technology spurs scientific advance. Much military R&D works this way.

Corporations are more likely to invest their own money in R&D, and take the risks of commercialization, if they expect rapid market growth. Government policies can reduce these risks. Trade protection does so, along with financial subsidies, and government purchases of a company's products. Strong legal protections for proprietary technology make R&D more attractive. Some governments go so far as to give financial help to customers for new technologies (computers and industrial robots in Japan). But with new knowledge eventually becoming available everywhere and to everyone, those who use it fastest and most effectively will come out ahead in international competition.

U.S. industry has been falling behind in the use of new technical knowledge, in part because of slow product development cycles. In many fields, the Japanese are not only doing a better job of engineering than their American rivals, they are doing it faster. Speed in moving from research to production and the marketplace will be a major factor in competitive success in HTS, just as in industries like automobiles or semiconductors. American firms have also had trouble as production volumes rise, and been poor at incremental product/process improvements. Their production capabilities enabled Japan's semiconductor manufacturers to establish themselves in world markets and compete successfully with American firms that had the lead in many of the functional aspects of circuit design. Many of the manufacturing techniques needed for HTS electronics will be similar to those for semiconductor devices (and ceramics).

Still, at the level of R&D and product development teams, Japanese firms do not seem to operate in greatly different fashion from successful American companies. The differences that do exist are important, however:[17]

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[17] K. Imai, I. Nonaka, and H. Takeuchi, "Managing the New Product Development Process: How Japanese Companies Learn and Unlearn," *The Uneasy Alliance: Managing the Productivity-Technology Dilemma*, K.B. Clark, R.H. Hayes, and C. Lorenz (eds.) (Boston, MA: Harvard Business School Press, 1985), pp. 372-373. Also see the "Commentary" by J.L. Doyle, p. 377.

Box D.—Commercialization in Europe

Excitement over HTS has been high not only in the United States and [illegible] Prize for the initial discoveries went to two Europeans [illegible] together programs intended to support commercialization by domestic [illegible] that Europe has had in other high-technology industries, it seems unlikely [illegible] of HTS will proceed as rapidly as in the United States and Japan.

Direct government support for commercial R&D has been a tradition in [illegible] France began funding technology development in computing and [illegible] one of many policy tools brought to bear in search of an independent [illegible] many pumped a billion dollars into the computer industry during the 1970s [illegible] to help Siemens compete with IBM in European markets.

Since the early 1980s, renewed efforts to create a truly common market within the [illegible] munity (EC) have led to programs like ESPRIT (European Strategic Program [illegible] tion Technology). The purpose is to stimulate cooperation in precompetitive R&D [illegible] borders, and tighten linkages between universities and industry [illegible] importance to the technologies generated.

ESPRIT itself resulted in part from a widespread feeling that national programs had been unsuccessful. Periodic proposals for a joint EC industrial policy culminated, by 1980, in an attempt to [illegible] together a European strategy in electronics. The plan—never implemented [illegible] beginning of the current wave of inter-European cooperative R&D [illegible] developments as RACE (R&D in Advanced Communications-technology for Europe), [illegible] search on Industrial Technologies for Europe), and Eureka. The latter [illegible] mercial technology development than earlier efforts focusing on pre-competitive R&D [illegible] mostly in basic research.

Advocates of these programs claim that European governments, and European [illegible] tives, have finally begun putting aside their differences, that they are now [illegible] together in meeting American and Japanese competition. Whether or not [illegible] gles of Siemens and Philips to keep up with Japan in large-scale [illegible] may not be enough. The two companies, together with the West German and Dutch [illegible] have committed a billion dollars to a cooperative effort involving independent [illegible] At this point, the Megachip project, the latest in a long [illegible] governments to keep pace in microelectronics [illegible] of changes on past failures.

Many of the large European companies have long traditions of high-quality [illegible] had difficulty commercializing the results [illegible] HTS. So far, the only sign pointing [illegible] Hoechst patented thallium-containing copper oxide HTS [illegible] announcement) by scientists in Japan.[1]

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[1] Hoechst AG, Press Release, [illegible]

- American firms tend to proceed through a more analytical and sequential approach, one of narrowing down the alternatives. Japanese firms operate in a looser style, with more room for trial-and-error.
- Product development groups in the United States rely more heavily on engineers with narrow technical expertise; Japanese companies staff their development groups with greater numbers of generalists, including people from sales and marketing. They may also involve the firm's suppliers.
- Japanese companies use product development groups as a device to break down

> some of the rigidities in their corporate cultures—e.g., the seniority system—and to create a place where creativity can flourish. Many American firms would like to think they don't suffer from such problems, but probably do.

At the same time, all of the attributes of Japanese product development efforts can be found in *some* American firms. It is the more successful Japanese firms that are visible in the United States: we seldom hear about the failures.

HTS poses difficult technical challenges. Japanese companies will, no doubt, solve some of the purely technical problems before American firms. Japanese companies will also do well at scaling up HTS manufacturing processes. Some will succeed in defining profitable markets. In short, they will prove highly capable and competitive in HTS. And while many large U.S. corporations have been turning away from long-term, high-risk R&D—the kind of work that will be called for in commercializing HTS—the Japanese are making a major effort to show the world they can be as creative and innovative in science as they are in technology. It would be a grave mistake to assume that American firms will have a head start in HTS because of U.S. skills in research. The suddenness of the turnaround in microelectronics should have pounded home the message that both industry and Government will need to do things differently in the future.

APPENDIX 2A: R&D AND COMMERCIALIZATION: FOUR EXAMPLES

Ceramics for Heat Engines[1]

The U.S. Government has spent perhaps $300 million since the early 1970s pursuing ceramic engines. Much of the money has gone for applied research and development on components, and for demonstrations. Success has been elusive.

Over the past two decades, advanced ceramics have come into widespread use in electronics, as well as for specialty applications such as wear parts and cutting tools. Ceramics hold their strength at high temperatures much better than metals, but are brittle. If reliable ceramic combustors and rotor blades could be made for gas turbines, operating temperatures could be raised, making possible smaller, lighter, and more efficient powerplants. Possible defense applications include stationary power units and engines for tanks, trucks, and cruise missiles (ceramic components may never be reliable enough for manned aircraft).

In 1971, DoD's (Defense) Advanced Research Projects Agency embarked on a ceramics R&D program, funding mission-oriented work of interest to the Army and the Navy on ceramic gas turbines, as well as research into design methodologies for brittle materials. The DARPA program continued into 1977, with funding that averaged slightly over $10 million annually. The Army continued some ceramic engine work thereafter, but DOE (then the Energy Research and Development Administration, ERDA) soon emerged as the primary source of support for applications-oriented ceramics R&D.

The ERDA program, in which NASA also participated, aimed at a gas turbine engine for trucks, seeking better fuel economy. Gas turbines make more sense for trucks than for passenger cars, which operate most of the time at light loads, where turbines give poor fuel economy. However, the focus on truck engines did not last. In 1980, responding to a high-level political call for the "reinvention of the automobile," DOE created a new program—one that would demonstrate small gas turbines for passenger vehicles. Initially funded at $20 million annually, the incoming Reagan Administration sought to scale the effort back (along with other energy R&D); lobbying by industry contractors helped keep things going.

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[1] *Increased Automobile Fuel Efficiency and Synthetic Fuels* (Washington, DC: Office of Technology Assessment, September 1982), pp. 144-145; T. Whalen, "Development Programmes—USA," *Proceedings of the First European Symposium on Engineering Ceramics*, Feb. 25-26, 1985 (London: Oyez Scientific and Technical Services Ltd., 1985), p. 177; K.H. Jack, "Silicon Nitride, Sialons, and Related Ceramics," *High-Technology Ceramics: Past, Present, and Future*, W.D. Kingery (ed.) (Westerville, OH: American Ceramic Society, 1986), p. 259; *Ceramic Technology for Advanced Heat Engines*, Publication NMAB-431 (Washington, DC: National Academy Press, 1987); J. Zweig, "Deja vu—yet again," *Forbes*, Nov. 16, 1987, p. 282; "Case Studies of 'Flagship' Technology," prepared for OTA by W.H. Lambright and M. Fellows, Syracuse Research Corp., under contract No. H3-5565, Dec. 31, 1987, ch. IV; R.P. Larsen and A.D. Vyas, "The Outlook for Ceramics in Heat Engines, 1990-2010: Results of a World-wide Delphi Survey," Paper No. 880514, prepared for the 1988 International Congress, Society of Automotive Engineers, Detroit, Feb. 29-Mar. 4, 1988; *Advanced Materials by Design: New Structural Materials Technologies*, op. cit., ch. 2.

Recent Federal spending (for all structural ceramics R&D) has averaged about $50 million per year (figure 2A-1), but the turbine programs appear to have moved prematurely into development and demonstration, before establishing an adequate technology base. Industry cost sharing has been relatively low; companies that saw more value in the work presumably would be willing to kick in money at a higher level.

Rather than turbines, Japanese firms have put much of their effort into piston engines, both gasoline and diesel. While brittleness is a serious problem in ceramics for piston engines, it is easier to deal with than in highly stressed rotating blades. Moreoever, ceramics can be introduced incrementally, substituted for a few parts in an otherwise conventional design.

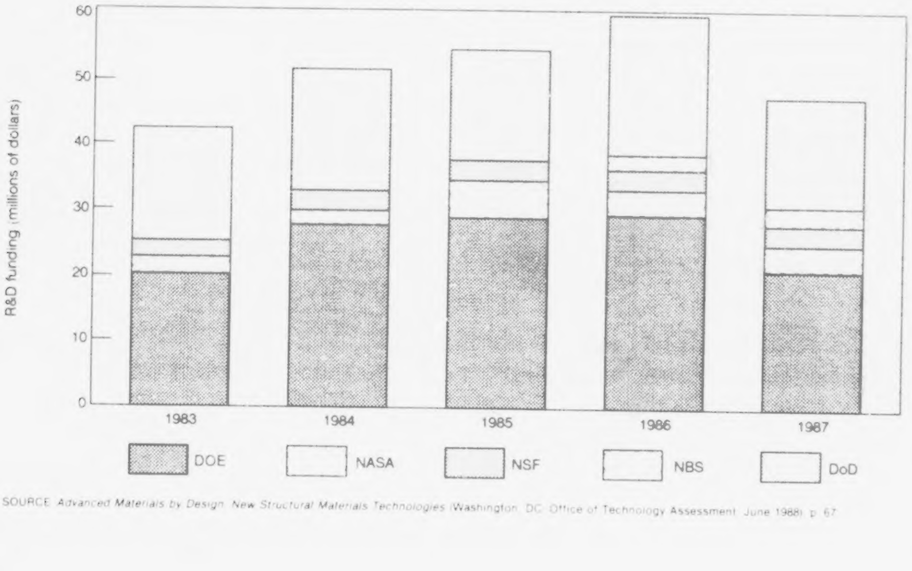
Some of the technical problems of structural ceramics overlap those that will be encountered in commercializing HTS ceramics. A stronger basic research effort in ceramics, rather than the demonstration projects of recent years, might have put the United States in a better position to commercialize the new superconductors.

Video-Cassette Recorders[2]

Beginning in the 1950s, and through the following decade, half a dozen and more Japanese companies raced to develop low-cost VCRs. Commercialization meant solving a long chain of tough engineering problems, so that VCRs could be produced cheaply with the features consumers wanted.

Helical scan video-tape recording technology—first patented by Toshiba (table 2A-1)—became a critical feature in VCRs, although Toshiba itself never capitalized on its early lead in helical scanning. Matsushita entered pilot production first, in 1973, but shortly withdrew, deciding its technology was not good enough. Two years later, nearly 20 years after the U.S. firm Ampex built the first

Figure 2A-1.—Federal Funds for Structural Ceramics R&D



SOURCE: *Advanced Materials by Design: New Structural Materials Technologies* (Washington, DC: Office of Technology Assessment, June 1988), p. 67.

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[2] *International Competitiveness in Electronics*, op. cit., pp. 70, 119-123, and 186-187; R.S. Rosenbloom, "Managing Technology for the Longer Term: A Managerial Perspective," *The Uneasy Alliance: Managing the Productivity-Technology Dilemma*, op. cit., p. 297; R.S. Rosenbloom and M.A. Cusumano, "Technological Pioneering and Competitive Advantage: The Birth of the VCR Industry," *California Management Review*, vol. XXIX, summer 1987, p. 51.

Table 2A-1.—Chronology of Video-Tape Recorder Developments

1951	R&D begins at RCA
1953	RCA demonstrates fixed head scanner
1954	Toshiba files patent applications for helical scanning; prototype follows in 1959. (Earlier U.S. and European patents were never reduced to practice.)
1956	Ampex introduces broadcast model videotape recorder (VTR) with rotating scanning heads. (VTRs use reel-to-reel tape, rather than cassettes.)
1958	Several Japanese firms, including Sony and Matsushita, embark on R&D directed at VTRs for consumer markets; RCA drops its work on consumer model VTRs
1962	Sony introduces its first helical-scanning VTR, intended for institutional markets (business and industry, schools); JVC follows in 1963
1969	Sony announces first video-cassette recorder (VCR), replacing reel-to-reel tape with a cartridge
1970-71	Ampex Instavideo camera/recorder system shown in prototype form—never marketed because of production problems.
1971	Sony U-Matic marketed for institutional use at $1000
1971	RCA resumes VTR R&D, drops out again in 1974
1972	JVC develops prototype of its VHS system
1973	Matsushita enters pilot production with a consumer VCR, but withdraws after a few months
1975	Sony introduces Betamax for home use
1976	JVC brings VHS recorders to market.
1988	Sony to begin selling VHS machines alongside its lagging Betamax system.

SOURCES: W.J. Abernathy and R.S. Rosenbloom, "The Institutional Climate for Innovation in Industry: The Role of Management Attitudes and Practices," *The 5-Year Outlook for Science and Technology 1981: Source Materials*, volume 2, NSF 81-42 (Washington, DC: National Science Foundation, 1981), p. 407; R.S. Rosenbloom, "Managing Technology for the Longer Term: A Managerial Perspective," *The Uneasy Alliance: Managing the Productivity-Technology Dilemma*, K.B. Clark, R.H. Hayes, and C. Lorenz (eds.) (Boston, MA: Harvard Business School Press, 1985), pp. 317-327; R.S. Rosenbloom and M.A. Cusumano, "Technological Pioneering and Competitive Advantage: The Birth of the VCR Industry," *California Management Review*, vol. XXIX, summer 1987, p. 51.

broadcast recorders (the size of a closet and selling for $50,000). Sony's Betamax opened the consumer market.

That an American firm produced the first videotape recorders for broadcast applications was close to irrelevant. The Betamax represented the fourth generation of Sony's engineering development—the seventh generation if the company's earlier industrial and institutional models (e.g., the U-Matic, which appeared in 1971) are included. Japanese companies, competing fiercely with one another, persisted with the VCR for years, in the face of many disappointments.

It may be true in a narrow sense to say that the United States invented the videotape recorder and the Japanese commercialized it. But in fact, some 15 companies — American, Japanese, European—demonstrated 9 different technical approaches to home video in the early 1970s. It took many years of money and manpower commitments by Japanese companies to win the race, and a great deal of highly creative engineering—focusing on manufacturing, as well as product design. Once the VCR became a commercial reality, competition centered on cost reduction, better image quality (where manufacturers of magnetic tape made major contributions), and longer recording and playing times.

Firms like Zenith and RCA—which now put their labels on foreign-made machines—never pursued consumer VCRs with the doggedness of the Japanese. After about 1980, no American company could have entered without some sort of breakthrough—a product that would have opened a new round of competition. The Japanese were simply too far down the learning curve. South Korean firms were in a different position: with wage rates well below those in Japan, they had potential cost advantages. When the Japanese refused them licenses, Korean firms developed their own VCRs.

The essential ingredients in Japanese success? First, willingness to make long-term investments in risky and expensive product development efforts. Second, the manufacturing capability to mass-produce precision electro-mechanical components such as the helical read-write heads that proved a key in turning the video-tape recorder into a household product. Commercialization of the VCR exemplifies the kind of incremental improvement and market-oriented engineering that the Japanese have been so good at.

Is the VCR story exceptional? Not really, and certainly not in the context of consumer electronics, an industry that had stagnated in the United States by the mid-1970s. Price competition in traditional products like color TVs was fierce, imports were flooding the marketplace, and the stronger U.S. firms like RCA and GE were diversifying into other lines of business.

Still, the risks did not stop RCA from investing in the VideoDisc.[3] Indeed, the VideoDisc was a bold choice. If successful, it would have given RCA a unique product—something that none of its Japanese rivals had. In contrast, pursuit of VCR technology would have meant competing in a class of products that the Japanese plainly would be able to build cheaply and well. RCA managers knew from experience in color TV production how difficult this would be, particularly given the Japanese strategy of attacking consumer electronics markets worldwide (whereas RCA's consumer sales had been confined to the U.S. market).

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[3] M.B.W. Graham, *RCA and the VideoDisc: the business of research* (Cambridge, UK: Cambridge University Press, 1986). The company ultimately lost more than half a billion dollars.

MRI Systems*

Magnetic resonance imaging has been the biggest market for conventional superconducting technologies over the past few years. In 1987, two dozen companies worldwide sold a total of 500-plus MRI systems to hospitals and clinics. At roughly $2 million each, industry sales came to perhaps $1 billion. Both production and sales are concentrated in the United States. Commercialization took many years, following research showing that nuclear magnetic resonance (NMR)—a discovery made by physicists—could be a powerful tool for medical diagnosis.

To construct an MRI image, the patient must be placed within a strong magnetic field—commonly produced by an LTS magnet. A computer processes the resulting NMR signals, creating an image the physician can examine (like an X-ray). MRI provides better contrast and resolution, particularly for the brain and spinal cord, than competing diagnostic imaging techniques, including ultrasound and CT scanning.

During the middle 1970s, more than a dozen companies in the United States, Europe, and Japan began working to commercialize MRI. Some dropped out along the way. Others were bought by stronger firms, or merged with competitors. Japanese companies entered late, and have not been very active outside their home market.

European firms led the way in engineering development. The British company EMI built the first prototype in 1978, and Bruker, a West German manufacturer, followed the next year. Both these companies had prototype systems operating in clinical settings by 1981. Shortly thereafter, EMI decided to leave the medical equipment business, and sold its technology to a competitor. By the end of 1983, eight firms had commercial prototypes available—three American companies, four European, and one based in Israel.

Early in design and development—e.g., during the stage labeled alternative conceptual design in figure 3 (earlier in the chapter)—each firm faced decisions on its magnet system. The alternatives—permanent magnet, resistive (non-superconducting), superconducting—carried advantages and disadvantages in terms of factors such as initial and operating costs, as well as field characteristics like strength and stability. Most companies chose LTS magnets, with several pursuing conventional magnet designs in parallel. Because the design of the magnet affects image quality—a central concern in purchasing decisions by hospitals—feedback from the clinical studies and clinical testing stages (figure 3 played a vital role in refining prototype designs.)

This brief description illustrates, first, the ways in which research may enter the commercialization process. In this case, the R&D ranged from nuclear physics (the NMR phenomenon itself), to the medical studies demonstrating that MRI could be a valuable diagnostic tool, to computerized signal processing and superconducting magnet design.

MRI systems emerged as viable commercial products in 1984. It was only then that designs stabilized and production became relatively routine, at least in the leading companies. It took 38 years to go from scientific discovery (experimental verification of NMR) to marketplace success. Commercialization in the sense of engineering development spanned the years 1977 to 1984.

Regulatory approvals were an early hurdle. The U.S. Food and Drug Administration spent several years evaluating the new technology. Manufacturers had to estimate the effects of third-party payment policies on market growth: Would Government agencies responsible for Medicare and Medicaid give a quick okay to the new technology? Or would they delay? How about the big insurance plans like Blue Cross/Blue Shield? In fact, hospitals were not generally reimbursed for MRI services until late in 1985. Furthermore, with MRI systems costing several million dollars, State government certificate-of-need approvals became a precondition for many sales.

U.S. firms did not have the initial lead. Nonetheless, they quickly emerged in the forefront as the technology moved out of the laboratory and became a practical tool for medical diagnosis. A major reason for U.S. success was simply that this country is the biggest market in the world by far for medical equipment. That the U.S. economy is the world's

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Health Technology Case Study 27: Nuclear Magnetic Resonance Imaging Technology (Washington, DC: Office of Technology Assessment, September 1984); "Superconductive Materials and Devices," Business Technology Research, Wellesley Hills, MA, September 1987, pp. 38-50.

largest and most diverse is both an advantage and a disadvantage for American firms. They are at home here, but their domestic markets are a magnet for foreign firms—who may be willing to lose money in the United States for the sake of learning and experience.

Designing and developing a new product from scratch, as in the case of the first MRI systems, represents a major corporate commitment. An all-new product takes much more time and money than the incremental redesigns, improvements, and new models that come later and constitute most of the routine work of product/process development. The all-new product (or manufacturing process) will also, in the ordinary course of events, depend more heavily on new knowledge—e.g., research results. Feedback from the R&D laboratory and the marketplace remain important even for routine development work, however. Once the medical community accepted MRI, competing firms quickly began differentiating their products through stress on image quality, good service, and reliability.

LTS Magnets[5]

Federal R&D, much of it for high-energy physics experiments and research into nuclear fusion, underlies development of the LTS magnets found in MRI systems. Wound with cable made from niobium-titanium alloy filaments embedded in a copper matrix, and cooled with liquid helium, the magnet accounts for up to a quarter of the cost of an MRI system.

Until MRI markets began to grow, most superconducting magnets were custom-designed for scientific equipment. The late 1960s saw the first major application of niobium-titanium, a bubble chamber built at Argonne National Laboratory. A much larger federally supported project—the Tevatron particle accelerator, completed in 1983 at the Fermi National Accelerator Laboratory—consumed more than 30,000 miles of niobium-titanium wire for its nearly 1000 magnets. Most of the wire came from Intermagnetics General Corp. (IGC), established by several former General Electric employees in 1971.

IGC and other small, specialized firms had begun moving into LTS as major corporations—Westinghouse as well as GE—withdrew, finding that market growth did not live up to their expectations. Development of the processing techniques for LTS magnet wire was a lengthy and complex task, one that would have taken much more time without the demand provided by the Tevatron. Private firms drew on the publicly supported technology base, and also helped to extend it, as they developed the know-how needed for manufacturing LTS wire and cable (the Fermi Laboratory designed and built the magnets internally).

It took many years to raise the critical current densities of niobium-titanium wire to the levels needed for the Tevatron and for MRI. The task hinged on the relationship between fluxoids (each of which contains a magnetic flux quantum)—a matter of physics—and the microstructure of the wire. Through careful microstructural control—specially tailored sequences of wire drawing and heat treatment—metallurgists and materials specialists were able to create fine dispersions of second-phase particles. These particles pin the fluxoids, keeping them from moving. The pinning can raise the critical current density—hence current carrying capacity—by 10 times or more. R&D aimed at optimizing the processing technology began in the late 1960s, and still continues, with engineering development guided by theoretical understanding.

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[5] D. Larbalestier, et al., "High-field Superconductivity," *Physics Today*, March 1986, p. 24; L. Hoddeson, "The first large-scale application of superconductivity: The Fermilab energy doubler, 1972-1983," *Historical Studies in the Physical and Biological Sciences*, vol. 18, 1987, p. 25; "Superconductive Materials and Devices," op. cit., pp. 33-61; "Technology of High Temperature Superconductivity," prepared for OTA by G.J. Smith II under contract No. J3-2100, January 1988; "Government's Role in Computers and Superconductors," prepared for OTA by K. Flamm under contract No. H3-6470, March 1988, pp. 56ff. Also see app. B.

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Chapter 3

Superconductivity in Japan and the United States

SUMMARY

The first 10 weeks of 1988 saw the discovery of two more copper-oxide based superconducting materials—one with bismuth as a critical ingredient, the other thallium. These two compositions—both with critical temperatures in the range of 100 degrees Kelvin—joined those containing rare earth elements (e.g., lanthanum, yttrium) that scientists around the world had been studying for a year. Laboratory resources had been heavily committed to the yttrium-barium-copper-oxide family—the so-called 1-2-3 superconductors—and the scientists had been making good progress in improving current densities and learning to make thin films. Then, all of a sudden, two entirely new compositions—equally complex, five elements in each, partially understood structures and phase diagrams. Two new worlds to explore. Heaven for the scientist (though more sleepless nights). Hell for the businessman.

Business planners and government strategists—at General Electric and Sumitomo, MITI and the Pentagon—now faced still more choices. Superconductors came in at least three varieties:

1. The old, low-temperature superconducting (LTS) materials—metal alloys like niobium-titanium, well understood but calling for cooling to near liquid helium temperatures—might still remain the material of choice for some applications. Very sensitive detectors of enemy submarines or brain waves might have to be operated at liquid helium temperatures in any event, to get noise levels down.
2. The 1-2-3 ceramics—brittle, not very stable, but with properties that people had begun to understand.
3. The latest high-temperature superconducting (HTS) compounds—those containing bismuth or thallium—still a mystery, but potentially easier to work with and perhaps having better combinations of properties than the 1-2-3s.

Then there is the fourth category—everything as yet undiscovered.

With no theory, only enlightened empiricism to guide the search, not even the biggest laboratories can explore all the possibilities. Choices must be made, priorities set, resources allocated. For a company, 50 people working on HTS means 50 people who cannot work on other projects that might, in the long run, be equally important.

This chapter is about those choices, and how they are made, in U.S. and Japanese companies, and in Japan's Government. Chapters 4 and 5 deal with the choices facing the U.S. Government.

Corporate managers in the United States and Japan look at the world differently. In seeking strategies for profits and growth, they make different kinds of choices, set different priorities, because they operate in contrasting economic, political, and social environments. Companies that do business on a global scale—IBM, Du Pont, Nippon Steel, Hitachi—may have much in common in their view of the world, but there are important differences between them as well. It may be a cliché to say that Japanese firms put more weight on growth and market share than on short-term profits, but it is true, and it makes a difference in R&D strategies, business plans—the entire array of competitive choices. The U.S. startups, financed with venture capital, that sprang up during 1987 have no counterparts in Japan. Nor do the small LTS specialists mentioned in the preceding chapter. Japan's joint government-industry R&D projects—a fixture of that country's industrial and technology policies—have no counterparts here.

Business planners must decide how many people and how much money to put toward superconductivity. They must decide how to spend that money, and what kind of people to assign. Is it too early to think about applications? Does it make sense to continue exploring LTS technologies? Managers in the United States and Japan have made diverging choices:

- A few large American companies are pumping substantial resources into HTS. But many other U.S. firms—organizations with the resources to pursue HTS if they wished—have taken a wait-and-see attitude. They may have a few people working on HTS R&D, but mostly just to keep track of the technology.
- Most of the effort in the United States is going toward research. American managers believe HTS should remain in the laboratory until more scientific knowledge is in hand.
- Perhaps a dozen large, integrated Japanese multinationals—manufacturers not only of electrical equipment and electronic systems, but of ceramics, glass, and steel—are pursuing multi-pronged R&D strategies in superconductivity. As in the semiconductor industry, these resource-rich companies could prove potent rivals for smaller American firms hoping to stake out a position.
- Japanese companies are conducting research but also thinking about applications. They are putting more effort than U.S. firms into thinking through what HTS might mean for the company's strategy. In general, managers in Japan believe that HTS is closer to the marketplace than do American managers. They also see HTS as a means of creating new businesses, while American managers are more likely to view it in the context of their existing business. The breadth of the Japanese effort substantially exceeds that of the United States.

Managers in the larger American companies believe that if HTS takes off, they will be able to catch up or buy in. Japanese managers want to move down the HTS learning curve in real time. They believe that advantages established now will last. Scientists, managers, and venture capitalists involved in the HTS startups in the United States believe the same thing, but they are few, small, and weak compared with the Japanese companies.

Taken as a whole, the U.S. approach—driven by the need to show financial paybacks in the short term—could leave American industry behind Japan within a few years. Such an outcome is not assured. HTS could languish in the research laboratories. Or HTS could evolve like the laser industry—never quite matching the expectations of the enthusiasts, driven heavily by military needs, lacking the revolutionary impacts of the computer or the semiconductor chip.

On the other hand, HTS could grow and spread and expand like the digital computer. Computers—especially the microprocessors and single-chip microcomputers found in microwave ovens and TV sets, banking machines and machine tools, Chevrolets and 767s—have penetrated innumerable products and manufacturing processes. The same could eventually happen with HTS technologies.

American companies, by and large, have taken the conservative view; Japanese companies have taken the optimistic view. If technical developments in HTS proceed as swiftly over the next 2 or 3 years as they did during 1987, then Japanese companies that have been laying the groundwork for commercialization will be in a stronger position.

Superconductor fever has swept through Japan's Government too, with ministries vying with one another for the lead in policy. The picture has now stabilized, but 1987 saw many actors seeking center stage—and few signs of the coordinated, monolithic policy machine that some Americans still think of as Japan, Inc.

CORPORATE STRATEGIES

What place does R&D have in the strategies of American firms? How do managers think about HTS? How does the business culture in the United States differ from that in Japan? Effective government policies depend on an understanding of the attitudes and practices of managers, the forces that condition their decisions. As it happens, there is substantial truth to the commonplace observation that Japanese managers take a longer view than Americans. This difference shows up in R&D decisions on HTS.

For years, American firms have been criticized for short-sightedness.[1] Managers are under pressure from Wall Street and institutional investors (pension and mutual funds, insurance companies) to show high and increasing quarterly earnings. Failure to do so can lead to a loss in stock values, and vulnerability to hostile takeovers. Jobs and egos are on the line, so the argument goes; few chief executive officers or division heads can survive many mediocre quarterly reports.

Techniques used by American managers for evaluating investment alternatives—discussed in the next section—reinforce the pressures to sacrifice long-term opportunities for short-term profits. Instead of investing in R&D that will increase their firm's storehouse of proprietary know-how, managers cut R&D to reduce costs. Instead of investing in new plant and equipment to increase productivity and flexibility, they slash payrolls, keep the old equipment running while spending no more than absolutely necessary on maintenance, and move labor-intensive production offshore or to the Sunbelt. Rather than putting money into core businesses, managers diversify (from steel to real estate, from manufacturing to services), buy up other companies rather than build their own, and seek paper profits. The picture may be a caricature, but it has a good deal of truth in it.[2]

How have these pressures affected corporate decisions on HTS? What other factors enter into R&D decisions? How, specifically, do U.S. managers view HTS compared with their Japanese counterparts? The next section of this chapter examines the R&D strategies of American firms. Later sections turn to Japan.[3] The findings in brief:

- American managers have been notably more reluctant to commit resources to HTS —a technology with highly uncertain prospects. They view profits as lying well in the future.
- Japanese executives, in contrast, seem confident that investments now will pay off—some time and in some way. Their view of the future is quite a different one from that of American managers.

These contrasting views reflect the business environments and investment climates in the two countries—indeed, the entire complex of factors that affects management decisions.

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[1] More than a dozen years ago, an experienced U.S. R&D manager wrote that " . . . the root cause of the present and future decline of U.S. technological prominence is a temporal mismatch between the natural pace of innovation and the time horizon of most U.S. industrial corporations . . . . this root cause is overlooked by the managers of major U.S. industries because they have a warped set of values"—R.D. Dean, Jr. "The Temporal Mismatch—Innovation's Pace vs Management's Time Horizon," *Research Management*, May 1974, p. 12.

A recent survey of nearly 140 U.S. companies found "greater emphasis on near-term lower-risk results-oriented work" in their R&D—"Trends in the Chemical Industry," Results of the March-May, 1987 Survey of ACS Corporate Associates. (ASC is the American Chemical Society. The survey covered corporate members from other industries as well.)

For 1988, the National Science Foundation has forecast the lowest rate of real, inflation-adjusted growth in R&D since 1977. Even the Electric Power Research Institute, financed by regulated utilities, evidently feels many of the same pressures as publicly owned corporations. According to the Institute's president, "We now must clearly demonstrate that there is value in what we are doing and that it falls in an acceptable business time frame. This is a remarkable difference from when we started" [1973]—"EPRI's New President Looks to the Future," *New Technology Week*, Feb. 1, 1988, p. 8.

[2] A typical example: Tektronix, a leading manufacturer of instrumentation and computer work stations, will fire 1,000 white-collar employees "in a bid to boost earnings." When the company announced that it would close down some R&D projects, and scale back its marketing and sales staff, a stock market analyst said, "They're addressing the right issues." See J.P. Miller, "Tektronix Plans To Dismiss 6 percent Of Its Workers," *Wall Street Journal*, Mar. 7, 1988, p. 12.

[3] Most of the information on company views of HTS comes from interviews in the United States and Japan during late 1987 and early 1988, and from surveys of U.S. and Japanese firms. The U.S. National Science Foundation, through its Tokyo office, conducted the survey of Japanese companies for OTA.

R&D and Business Planning in the United States

Funding Decisions

American firms approach R&D much like any other investment. With some exceptions, a decision on individual R&D projects or divisional R&D budgets will be viewed in the same light as a decision to invest in new production equipment, acquire another company, or sell the firm's Manhattan headquarters and move to New Jersey. Box E describes the process.

R&D carries higher risks than many other corporate investments, in the sense that outcomes are less certain. Moreover, the projects with the greatest uncertainty tend to be those with longer payback periods. As explained in box E, such projects must promise exceptionally large rewards, or the investment money will go elsewhere.

Research that loses out in private corporations might nonetheless benefit the country as a whole. If no one company can reap the rewards, none may invest. That is why the Reagan Administration has continued relatively liberal funding for basic research, even though cutting back on more applied work. Companies do little basic research because, from their perspectives, it does not pay. But the social returns from a portfolio of such investments can be great.

R&D Management

American companies normally engage in R&D to support existing business activities, or those that have emerged from reasonably careful planning exercises. Even the two remaining giants of U.S. corporate research—IBM and AT&T—seek, in their own quite different ways, to guide and manage R&D in support of overall corporate goals.

Oriented toward results, American executives see corporate R&D as an activity to be guided by the firm's overall objectives. Only rarely do they look to R&D as a means of uncovering wholly new business opportunities. When they do, they tend to seek home runs (like the Xerox copier or Polaroid photography) rather than the incremental advances that have a central place in Japanese corporate strategies.

Du Pont would dearly love another product like Nylon, and Intel another invention with the impact of the microprocessor, but who knows where these might come from? Inventions cannot be planned, and no company will spend much money on an unguided search. Furthermore, in big organizations with ample R&D budgets, projects that might be exciting technically can get lost in the corporation's grand strategy. Even though they might promise high rates of return, if the overall market looks relatively small, a big company may not be tempted. Low-temperature superconductivity provides a number of examples, and HTS will probably bring more.

Most firms give their R&D managers latitude in initiating work on their own, hoping for results that will eventually contribute to the bottom line.[4] Individual managers, moreover, do not always follow corporate policy. Working-level people bootleg research that might not be approved higher up. Top management normally lets project leaders and departmental managers follow their own judgment, so long as not too much money is involved. Star researchers, likewise, may be left alone to pursue their hunches and intuitions (which is how HTS was discovered in IBM's Zurich laboratory). A few companies let people spend some fraction of their time—usually small—following personal research interests.

Such policies tend to be pursued for reasons of morale. They help create a more comfortable environment for industrial scientists, a more academic setting. If the results bring in money for the company, this will normally be viewed as a lucky accident; in most U.S. firms, most R&D scientists and engineers work within carefully managed groups, on projects that corporate management first approves and then monitors.

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[4] This latitude seems increasingly circumscribed. For instance, many U.S. research managers must take such decisions as whether to spend, say, $250,000 to join an R&D consortium, all the way to the top of the management hierarchy. See "Roundtable: Physics Research In Industry," *Physics Today*, February 1988, p. 54.

[illegible]—R&D in U.S. Corporations

Many large American firms have turned away from corporate research. Some—U.S. Steel in the 1970s—have closed their laboratories. Others—Exxon comes to mind—have scaled them back dramatically. GE has shed the Sarnoff Laboratory inherited from RCA.[1] The staff of AT&T Bell Laboratories has shrunk by 5,000 people since divestiture. Even those laboratories that have continued to expand—for example, IBM's research arm—find themselves under renewed pressure to support development projects for the operating divisions. None of this is necessarily bad. Linking research with engineering and production is a perennial problem. Much of the success of Japanese corporations in world markets can be traced back to effective management of product and process development.

But things have, in fact, changed in the United States. R&D of a sort that companies funded during the 1960s or 1970s might not go forward today. It may even be possible that American managers have lost sight of their company's long-term interests—misled by oversimplified techniques for investment analysis, the short-term myopia alluded to earlier in this chapter.

Present-Value Methods

Most American companies make most decisions on R&D spending in accordance with straightforward financial criteria: they compare proposed R&D investments with other possible uses of their funds. If the R&D promises a high enough payback, they will pursue it. If not, they will do something else with the money.

This means that in many if not most American corporations, R&D managers must justify their budgets in much the same way as operating managers. All use standard methods of financial decision making, normally based on the present value of cash flows anticipated from the investment. Companies estimate expenses and revenues over time, then "discount" them based on a factor that depends on the firm's cost of capital. For an R&D project, the expenses correspond to those listed in table 2 (in ch. 2): they begin with the R&D itself, and continue on through commercialization. Revenues begin when the product reaches the marketplace. The discount rate used in these estimates depends on prevailing interest rates in capital markets, along with many firm-specific factors.[2]

The basic decision rule is simple: accept projects that show a positive present value, reject those with negative present values. Put another way, the company would invest in projects with predicted rates of return greater than the company's cost of capital. Of course, the future is always uncertain—and when it comes to R&D, it can be highly uncertain. But the method permits firms to compare uncertain alternatives.

Two conclusions follow directly:

1. Investments that show positive cash flows in early years appear more desirable than those with cash flows equal in magnitude but later in time. Projects with late payouts—common in R&D—will be more difficult to justify. The longer sales revenues are delayed, the greater those revenues must be.
2. The higher a firm's cost of capital, the more difficult it is to justify any project. Managers typically handle the uncertainty associated with longer-term projects by increasing the discount rate they apply. This puts a double hurdle in front of long-term R&D projects.

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[1] GE has a long tradition of excellence in research, but even so has achieved many of its greatest successes over the years through turning other people's inventions into commercial [illegible]. One example: improvements to Parsons and Curtis steam turbines so that they became practical for driving electrical generators. [illegible] "R&D in General Electric," R&D Pioneers Conference, Hagley Museum and Library, Wilmington, DE, Oct. 7, 1985. Wise, [illegible] historian, emphasizes the longstanding ties between product development and research in the company, illustrated by the [illegible] laboratories with their extensive evaluation and testing of insulating materials. [illegible] GE's research style as emerging from a conflict between two schools of thought, represented by Thomas Edison and Elihu Thomson. Thomson emphasized teamwork and evolutionary development, an approach that quickly became dominant. Nonetheless, the visionary [illegible] continued to have followers at GE. According to Wise, [illegible] turn toward basic research—a swing which seems to have reversed in the last 5 to 10 years—can be seen as [illegible]

[2] On costs of capital, including differences between the United States and Japan, see *International Competitiveness in Electronics* (Washington, D.C.: Office of Technology Assessment, [illegible] 1983), ch. 7.

Return on investment as a decision [illegible] measure of corporate performance originated within Du Pont early in the [illegible] and quickly replaced such ratios as [illegible] the primary financial measure in use. See R.S. Kaplan, "Accounting [illegible] of Cost Accounting Systems," *The [illegible] Productivity-Technology Dilemma*, K.B. Clark, R.H. Hayes, and C. Lorenz (eds.) (Boston, MA: Harvard [illegible]

Implications

Why don't American companies adopt modified decision criteria for long-term research? For instance, they might set aside some proportion of their R&D budgets for uncertain but promising projects of long duration. In fact, some do. But most U.S. managers believe the standard approach remains adequate. These methods provide a rational way to compare alternative investments. By taking care of today and tomorrow, one automatically takes care of the far future.

In this view, potential payoffs far off in time *should* be heavily discounted. What if a company ignores a new technology until the payoffs become obvious—by which time it may be too late to enter and compete effectively? To financial analysts, the answer is plain. The firm's cash flow projections should have taken all this into account. If the company made a mistake, it must have been in the numbers used, not the decision methods. At the same time, it seems plain that these methods—followed too slavishly—can become a recipe for risk avoidance. Japanese decisionmakers proceed differently; in HTS, they are already investing in applications-related projects, some of which simply couldn't get started here.

The characteristic U.S. approach has a positive side, as well. The methods are straightforward; they can be applied in consistent fashion to projects of all types. No company can undertake all the investments that come up for management review. Choices must be made. Taking one course of action closes off others. Present-value methods force managers to adopt common criteria for considering each alternative. Few projects will be accepted based only on advocacy by powerful managers; they too must show financial figures that pass muster. Of course, everyone tries to present numbers that back up their judgments, and R&D managers have a good deal of discretion in how they spend their money. But the point is that the financial figures can be scrutinized by critics as well as advocates. The rules are clear.

Research: An Endangered Species

Only a few American corporations continue to fund much long-term, basic research. Most of them fall in one of two groups:

- Those in regulated (or formerly regulated) industries—notably telecommunications. Bell Laboratories, AT&T's justifiably famous research arm, falls in this group, along with Bellcore, the R&D organization for the new regional telephone companies. Until recently, the costs of research at Bell Laboratories could be passed along to the operating companies—which, in turn, were able to justify them in their own regulated rate structures. In essence, the costs were passed directly through to ratepayers.
- Companies that historically have enjoyed quasi-monopoly positions in their industries—most notably, IBM. For many years, IBM was relatively free from competition, and able to devote large sums to R&D without much pressure from upper management to show immediately useful results.

Conditions are changing for both groups of companies. AT&T has been stripped of the operating companies, and deregulation has exposed its long-distance telephone services to new competition. The company faces many strong rivals in its other businesses—computer and communications hardware. IBM's world has likewise become more competitive, a trend that will continue as mainframe computers decline in relative importance in the marketplace.

How long will these companies continue to support basic research? Other corporate laboratories that 10 or 20 years ago pursued well-regarded basic work have turned away. In an ever more competitive world economy, American executives have tended to view research as an expense rather than an investment. The last of the industrial laboratories doing fundamental research in the United States could vanish, perhaps even by the turn of the century. If this happens, either government will have to pick up more of the risky, long-term R&D, or the United States will have even more trouble in staking out technological advantages vis-a-vis its economic (and military) rivals.

These practices follow quite naturally from accepted business practices in the United States. They are part of the received wisdom—wisdom that says the rare inventive genius will in any case strike out on his or her own, founding a new company to exploit whatever is really new.

U.S. Strategies in High-Temperature Superconductivity

As the survey results in box F indicate, at least 28 American companies are spending at the level of $1 million or more on superconductivity R&D. On the average, they have nearly 20 professionals at work. Many of the companies working in HTS have LTS experience. During 1987, most of HTS R&D went toward understanding the 1-2-3 ceramics, and toward processing-related work. The high proportion of scientists compared to engineers reflects the basic character of the research.

A number of American companies, big and small, have been conducting R&D on superconductivity—and perhaps producing LTS wire or magnets—for two decades or more. Some attempted to commercialize LTS products, later to scale back or abandon their work. In the 1970s, LTS-based Josephson junctions (JJs) excited considerable interest in U.S. electronics and computer companies. The enthusiasm faded, and much of the U.S. work was eventually dropped. (The Japanese persevered with JJs, as discussed in box J, later in chapter.) In other cases, companies like GE—with its line of medical imaging equipment incorporating LTS magnets (ch. 2)—have gone on to become major forces in the marketplace. Still, some of the Americans with experience in LTS view the new ceramic superconductors with considerable skepticism. Earlier disillusionment may have affected the current strategic posture of some American firms.

Indeed, many U.S. R&D managers feel it is too early even to think about applications of HTS. They think much more research will be needed to characterize the new materials. Moreover, they believe that commercial payoffs are likely to be in the distant future. Media hype has had little influence on them. These views affect funding for HTS R&D.

But if research in HTS is called for, who, on the American side, will do it? With few exceptions, U.S. industrial R&D laboratories avoid science. Their job is to support the operating units. U.S. industrial research grew rapidly during the early 20th century—led by companies like GE (which established a corporate R&D facility in 1900), Du Pont (which followed in 1902), AT&T (1911), and Eastman Kodak (1912). But decline has set in, for reasons that range from corporate decentralization to the shortening of time horizons. Today, few American corporations pursue much basic research with their own funds. Thus, when American managers state that HTS belongs in the laboratory, they often mean someone else's laboratory.

Two strategic scenarios, then, encompass most American firms:

1. The first includes the companies that have taken a careful look at how HTS might affect their businesses, assuming continuing advances in the technology. Such assessments often entail a complete review of the firm's product lines—a process some firms have begun by revisiting earlier evaluations of LTS.

   At this stage, such an assessment is no easy task, given the uncertainties. No one can predict which of the new families of

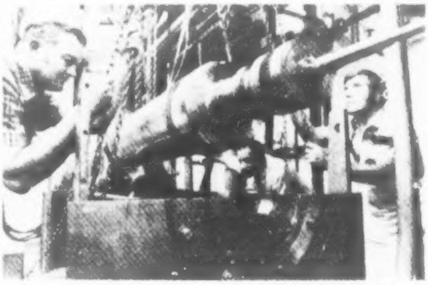

Photo credit: Westinghouse

Rotor for protoype LTS generator

Box F.—Superconductivity R&D in U.S. Companies

Between January and June, 1988, OTA surveyed 55 U.S.-based companies with significant R&D efforts underway in HTS (i.e., more than a handful of people tracking the technology). The survey was not exhaustive; the results should be viewed with caution, as a lower bound on U.S. activities. Nonetheless, it probably captures most of the R&D in HTS in startups and large firms.

Level of Commitment

- The 55 firms had approximately 625 scientists and technicians at work on superconductivity R&D (both LTS and HTS), either part-time or full-time.
- In total, the 55 firms expected to spend $97 million on superconductivity R&D in 1988.
- Twenty-eight of the firms have a current annual commitment of $1 million or more for superconductivity R&D. Of those, 16 are spending in the $1 million to $2 million range; 9 from $2 million to $5 million; and 3 firms are spending $10 million or more (none fall between $5 million and $10 million).

Types of Firms

- The 55 companies fall into the following broad categories: firms whose main business has been LTS materials and/or applications, or other advanced materials (including ceramics) (19); recent startups (5); defense and aerospace firms (12); and others—many of them large manufacturers with diversified product lines, and firms in industries including electronics, telecommunications, and chemcials (19).
- The 1987 revenues of the companies surveyed range from zero (for a new startup) to $100 billion.
- Average 1988 R&D spending on superconductivity in the four categories is as follows:

LTS and advanced materials	$1.1
startups	$1.7
aerospace/defense	$1.1
other	$2.9

R&D Activities

- Of the 625 people engaged in superconductivity R&D, three-quarters are scientists/engineers, the rest technicians. The scientists/engineers divide as follows:

physicists	43 percent
materials scientists	30 percent
chemists	15 percent
electrical engineers	12 percent

- Most firms have some work on materials characterization underway, but the bulk of the basic research is going on in a half-dozen large corporations.
- HTS is receiving much more R&D effort than LTS. Among the 28 firms spending $1 million or more, only 7 are directing more than 30 percent of their R&D efforts to LTS materials and applications, and these are mostly companies with extensive experience in LTS (including defense contractors).

Other Observations

- Less than half of the 55 firms are involved in some sort of domestic cooperative R&D activity in superconductivity; others expect to get involved in the next 2 years. For a number of firms, this activity is limited to cooperation with a single university or federal laboratory.
- Perceptions of where "the most significant HTS R&D is currently being done" centered heavily on IBM and AT&T. Also mentioned frequently were Stanford University, Berkeley, the University of Houston, Argonne National Laboratory, and Du Pont. When asked "which country is ahead in HTS R&D today," 22 of the 55 firms surveyed indicated Japan, and 7 indicated the United States; 14 companies said they viewed it as a tie or else gave the United States the lead in science and Japan the lead in applications. (The other 12 firms did not answer this question.)
- About half of the 28 companies spending at the $1 million and above level are getting some Federal R&D funds, as are a handful of the firms spending below that level. Many other companies expect to get some of their superconductivity funding from the Federal Government in the future (the survey predated most fiscal 1988 Federal contract awards).

materials might prove most useful, what kinds of problems the design of practical magnets or Josephson junctions might bring, whether three-terminal devices will emerge (ch. 2)—much less the costs.

There is a second problem, one creating even more uncertainty. Will somebody discover superconductivity at room temperature this year? Next year? In 2050? This makes all the difference for any economic evaluation. In fact, most U.S. companies have based their assessments on liquid nitrogen operating temperatures—an assumption leading to relatively pessimistic evaluations except for quite specialized applications. The typical view goes something like this:

- The primary need is for materials characterization, work that can be carried out (and is) at literally hundreds of academic, government, and corporate laboratories around the world.
- Our company could spend a lot of money on HTS without much chance of a breakthrough. Even then, the research would probably not result in proprietary advantage.
- In any case, the first applications are likely to be in defense systems, where cost constraints are less severe.
- Under these circumstances, the best strategy is to hedge the company's bets by tracking the science and technology worldwide, without investing heavily.

Such a strategy implies willingness to alter course if someone else makes a major breakthrough (not necessarily in operating temperatures—a big increase in critical current densities might be enough for at least some U.S. firms). These companies—many of them currently spending at the $1 million to $5 million level (box F), and with perhaps a dozen people assigned to HTS—will keep a core group at work. But they are not ready to jump into the HTS R&D race.

2. The second strategic scenario includes those companies, most of them large, with strength in research and the ability to pursue HTS R&D on a significant scale. The list is short: AT&T, IBM, Du Pont, Bellcore, Westinghouse, GE, and a few others might be added, along with several major defense contractors.

   Here, the presumption that HTS should remain in the laboratory is not a bar. Of course, not even IBM or AT&T can do everything; these companies too face the choice of investing money and manpower in HTS or in alternative R&D projects. But HTS exerts a powerful attraction, not only on working scientists, but on those who manage research. Finally, for some of these companies, success in HTS R&D could have pervasive impacts on their businesses. In a company like IBM, which already maintains a portfolio of equally uncertain R&D—most with far less potential impact—HTS quite naturally gets a high priority. A few American firms, then, have 50 or 60 people assigned to HTS, and some work underway that verges on development.

There is also a third group, not large, consisting of startups with venture financing (see box G), plus other small firms.

Government money for R&D could pull a few more American firms into HTS R&D. But much of this money will go for defense projects. Even in companies that include military or space divisions along with other operations—as IBM, AT&T, and many other large corporations do—the two sides of the company normally operate largely separated from one another (ch. 4).

In summary, most U.S. companies have adopted a wait-and-see attitude toward HTS. They may have assigned a group of people to monitor developments. Perhaps they conduct research on a small scale. But few major U.S. firms have placed superconductivity among their top R&D priorities. The others see good reasons for their decisions, of course. Risks and uncertainties are high; judgments differ. But if HTS develops more rapidly than they anticipate, few U.S. companies will be able to respond as quickly as the aggressive Japanese firms that have already begun laying groundwork for commercialization.

Box G—Venture Startups in HTS[1]

Through the end of 1987, private venture capital funds in the United States had invested about $20 million in four startup companies aiming to exploit HTS. Several other startups were looking for venture funding. At least at first, the startups expect to work on proprietary HTS technologies that other companies can bring to market.

Interest in the possibilities of HTS has been widespread within the venture capital community; OTA estimates that an additional $50 to $60 million would potentially be available during 1988 if the right opportunities came along (i.e., significant new discoveries).[2] Most of the activity has centered around 10 or so venture capital firms that have succeeded in the past with long-term, seed capital investments—e.g., in semiconductor and biotechnology startups, including a number that today are cornerstones of their respective industries. The pattern in HTS resembles that in the early days of biotechnology: startups have been brainchildren of the venture capital community, rather than of capital-hungry scientist/entrepreneurs.

Investors have sought out university faculty with past accomplishments in superconductivity at schools like Stanford, MIT, and Berkeley. Startups like American Superconductor Corp. and Conductus Inc. are managed by members of the supporting venture-capital firms. The startups have gradually added staff, but so far have mostly worked with university patenting/technology licensing offices in lining up consultants and advisory boards, while searching for corporate partners to share the investments, aid in technology development, and stand ready to help with marketing. Each of the new firms has been funded by pooling money from several venture capital sources. None had a business plan at the time of initial capitalization.

Most of the venture capitalists interviewed felt that commercialization of HTS would take 5 to 10 years, with some specialized defense applications (e.g., sensors) perhaps coming sooner. Those who have plunged in state that they are in for the long term. They generally agreed that the large, integrated Japanese manufacturers will be at a substantial advantage when the time comes to move into production and marketing. This is the reason they have searched for partners among bigger companies here. None had taken on a European or Japanese partner, although all have been approached.

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[1] "Early Investments in High-Temperature Superconductivity," prepared for OTA by D. Sheridan under contract No. J3-2665, December 1987.

[2] In a recent survey, venture capital firms put superconductivity at the top of the list of potential investments that would excite the most interest during 1988—well above biotechnology, computer software, fiber optics, and robotics. See *High Technology Business*, March 1988.

Japanese R&D

As in the United States, the R&D strategies of Japan's corporations flow from more general managerial attitudes. In important respects, Japanese executives exhibit decisionmaking behavior that differs from that here. As noted in the preceding chapter, U.S. and Japanese management styles also show many similarities, particularly in high-performing companies, but some strategic choices that make sense in an American context may be incompatible with Japanese views.

Corporate Research in Japan

Patterns of industrial R&D have been changing rapidly in Japan. American firms, accustomed to advantages in technology, must also adapt—perhaps to being first among equals. Japanese firms have a tougher job. They are trying to catch up and take the lead—and trying to do so with people and organizations that, until recently, started by licensing and adapting foreign technologies.[5] This takes money, and Japanese industry has been willing to spend it.

Table 5—showing the rapid rise in business-funded R&D in Japan—demonstrates the strength of that commitment. Japa[illegible] see technology development as a key ingredient in com-

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[5] See *International Competition in Services* (Washington, DC: Office of Technology Assessment, July 1987), ch. 6.

Table 5.—R&D Funded by Business and Industry

	Business-funded R&D expenditures		
	1981	1983	1986[a]
United States:			
Billions of dollars	$35.9	$43.2	$58.2
As percentage of all U.S. R&D	50.0%	50.0%	49.8%
Japan:			
Billions of yen	¥ 4364	¥ 5451	¥ 7000
Billions of dollars	$19.8	$22.9	$41.6
As percentage of all Japanese R&D	72.9%	75.9%	77.8%

[a]Estimated.

SOURCE: *International Competition in Services* (Washington, DC: Office of Technology Assessment, July 1987), p. 205.

petitive strategies. While business-funded R&D in the United States has been going up almost as fast in real terms, the overall lead of the United States in private sector R&D stems simply from the greater size of the American economy; on the average, Japanese firms spend substantially more on R&D as a percentage of sales.

In earlier years, major Japanese corporations began by scanning the world for technology, often with the aid of Japan's large trading companies, as well as the government. When possible, they set one potential source for technology against another to minimize licensing costs. Japanese companies followed with engineering excellence, highly developed manufacturing systems, and carefully targeted marketing strategies—often competing aggressively at home before launching their export drives.

But the world has changed for Japanese companies. In many technical fields, they have reached parity with the West. American and European firms, in any case, are much more wary of licensing than even 10 years ago. There is little more for Japan to assimilate. Japanese firms must either wait for new ideas to appear elsewhere or step up their own research. Even for companies not pressed by increasing competition from newly industrializing countries like South Korea, the first choice is a recipe for disaster at home. Thus Japan's major corporations are working hard to generate new technical knowledge.

This search for proprietary technologies means more basic research.[5] As American companies turn away from relatively fundamental work, Japanese firms are turning toward it. Many American R&D managers give the Japanese little chance of accomplishing much, at least over the next 5 or 10 years. They view Japan's culture—and the organizational environment in Japanese firms—as hostile to creative research. Many Japanese would agree. Their engineers may be superb at painstaking product development efforts, but, at least according to the stereotype, research demands individuality and creativity—qualities discouraged in Japan.

This stereotype is greatly exaggerated: a closer look suggests that creativity in engineering—something the Japanese have amply demonstrated—differs little from creativity in research and in science. In fact, U.S. scientists and R&D managers directly involved in HTS research give their Japanese counterparts high marks for their work. Moreover, in related fields like ceramics, the Japanese already have the lead in commercialization.[7] While Americans still see Japan as lagging generally in science, there is no basis for complacency—and certainly not when it comes to superconductivity.

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[5] See, for instance, S.K. Yoder, "Japanese Launch Bid to Lead the World in Pure Science," *Wall Street Journal*, June 3, 1987, p. 26. Also P. Marsh, "The search for some home-grown heroes," *Financial Times*, July 6, 1987, p. 15, which quotes Tokyo University's Professor Shoji Tanaka, Japan's best-known superconductivity expert, as follows: "For a long time the Japanese people had the feeling they were behind in science. But now the inferiority complex is starting to vanish. We do have a relatively inflexible university system. But young people are changing and will force their professors to adopt different ideas." In searching for creative scientists and engineers to staff their research laboratories, Japanese companies are hiring more women and foreigners—M. Kanabayashi, "An Acute Shortage of Engineers Threatens Japan's Research Goals," *Wall Street Journal*, Oct. 15, 1985, p. 32; "Poor lab facilities hamper plan to attract foreign researchers," *Japan Economic Journal*, Apr. 16, 1988, p. 5.

[6] Industry and government in Japan have put considerable emphasis on the life sciences in their overall drive for research excellence. See *Commercial Biotechnology: An International Analysis* (Washington, DC: Office of Technology Assessment, January 1984), pp. 505-510. Later sections of this chapter discuss government policies in support of basic research in Japan.

[7] *High-Technology Ceramics in Japan*, NMAB-418 (Washington, DC: National Academy Press, 1984); *Ceramic and Semiconductor Sciences in Japan, 1987*, PB 88-122478 (Washington, DC: Department of Commerce, 1987); *Advanced Materials by Design: New Structural Materials Technologies* (Washington, DC: Office of Technology Assessment, June 1988).

Time Horizons

How about the longer term view that Japanese managers reputedly take? This stereotype holds up—as can already be seen in HTS. The reasons begin with notions of success and failure that differ substantially between the business cultures of the United States and Japan.

In the United States, management perceptions of the factors that determine the value of a firm's stock heavily influence decisions. When surveyed, U.S. managers rank profits (as measured by return on investment), and increase in share price, as their primary objectives. Japanese executives also view return on investment as important, but put it below another goal—market share—which appears no better than third in rankings by American managers.[8]

Furthermore, Japanese companies need not worry too much about the price their stock commands, given the way Japan's financial markets work. Equity remains less important than debt in corporate financing, and new stock issues are the exception in raising funds. The now-standard—and often oversimplified—arguments concerning costs of capital come into play here; plainly, on a present value basis, or indeed almost any reasonable criterion, lower costs of capital in Japan make long-term projects more attractive.[9]

Japanese companies, then, typically use different decision rules in evaluating investment alternatives. Managers in Japan see R&D as a means for maintaining or increasing market share, both at home and abroad, with market share a necessity for holding on to a competitive position in dynamic markets. Japan's rapid postwar economic growth, and success in exporting, has made market share the top priority; when sales are expanding rapidly, grabbing as big a share as possible, and holding on to it, become the key to profits.

The emphasis on growth reflects a belief among Japanese executives that only large companies can remain financially viable in international competition. Japanese industry has spawned few of the entrepreneurial startups so much a part of the scene in U.S. high-technology industries—although many policymakers in Japan would like to create a place for them.

Other factors and practices reinforce the view that growth is all-important. Larger Japanese companies historically have attempted to provide "lifetime" employment for a portion of their work force. Managers continue to view this as an obligation, and growth makes it easier to sustain employment. Layoffs tend to be seen as evidence of management failure, rather than—as in the United States—a consequence or symptom of economic downturn. This sense of obligation helps shape corporate goals and managerial behavior. Where American executives would slash payrolls, Japanese companies will often accept lower profits.

What does this mean for R&D? A continuous search for new products and new markets, including those that might not fit very comfortably into ongoing operations. Where American companies look to R&D to support existing businesses, Japanese companies are just as likely to see it as a means of creating new businesses. Where American firms look for home-run opportunities, their Japanese counterparts have been more willing to start small and grow new businesses gradually.

Government-Industry Relations

The antagonism with which so many U.S. corporate managers view government also contrasts with typical Japanese attitudes. American managers feel, by and large, that the Federal Government's role should be tightly circumscribed. Wherever possible, economic matters should be left to the private sector.

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[8] J.C. Abegglen and G. Stalk, Jr., *Kaisha: The Japanese Corporation* (Tokyo: Charles E. Tuttle, 1987), pp. 176ff.

While decision criteria in Japan certainly differ from those here, there is little consensus in the West on the extent to which Japanese firms rely on financial measures—or, more precisely, on what kind of measures they use, and for what purposes. See, for example, "Part Two Discussion Summary," *The Uneasy Alliance: Managing the Productivity-Technology Dilemma*, op. cit., p. 283.

[9] *International Competitiveness in Electronics* (Washington, DC: Office of Technology Assessment, November 1983), ch. 7.

Later sections of the chapter describe how government and business in Japan have worked together to promote HTS. Here, the point is simply that Japanese executives have relatively comfortable working relationships with government. Japanese managers tend to feel that employees of their government are competent and deserving of respect, even when they disagree vehemently on matters of policy.

Managers in Japan pay attention to goals and objectives announced by the Ministry of International Trade and Industry (MITI), or the Ministry of Finance. This does not mean they will necessarily follow the paths that MITI or other ministries attempt to lay down. Contrary behavior is common. On the whole, Japanese executives would prefer to go along with government, while working to mold policy in ways they regard as desirable.

R&D Management

Traditionally, Japanese R&D has focused on engineering, product/process development, rapid transfers to manufacturing. Despite the engineering perspective, managers put less weight on short-term outcomes, and show more willingness to invest in projects that will not yield positive cash flows until well into the future. Japan's national goal of technological independence pushes companies in the same direction.

Personal opinions by managers carry great weight—especially when the advocates of particular projects enjoy high standing as researchers or managers. One man's recommendation can lead to a major new R&D project in Japan—something that would be highly unusual in an American company.

Competition among Japanese firms combines with cultural characteristics to yield another contrast with the United States. Japanese companies tend to emulate one another; when one begins research in a field like HTS, others follow. Few executives will risk letting a direct competitor engage in R&D without investigating the subject themselves. Similarly, companies are uncomfortable at the thought of closing down a research program that others are continuing. For such reasons, Japanese firms spend a good deal of effort tracking their competitors' day-to-day R&D efforts. American companies, which tend to look to R&D for means of differentiating themselves, show more interest in products soon to hit the market.

Finally, given the way Japanese firms make decisions, it should be no surprise to learn that [illegible] stick with R&D efforts once begun. One executive commented in an OTA interview, "In Japan, we continue research projects unless persuasive reasons are mustered against them. In the United States, I get the feeling that projects are cancelled in the absence of good arguments supporting continuation. The difference is subtle, but important. We tend to be optimistic on research results; you tend to be pessimistic." Such attitudes may be remnants of an earlier time, when success came easier. Still, they contribute to the persistence that has been so important to Japan's accomplishments in commercialization.

The typical Japanese approach to R&D carries disadvantages as well as advantages. With little systematic guidance for comparing one project to others, and subject to the influence of strong personalities, Japanese firms risk bad decisions. This weakness could become more important in the future, given their lack of experience in managing fundamental research—particularly if companies follow one another down blind alleys. But for now, the freer hand that Japanese managers have in allocating resources to long-term, high-risk projects is a notable strength.

Japanese Strategies in High-Temperature Superconductivity

Japanese R&D managers, almost unanimously, see HTS as a revolutionary technology, one that promises radical change. Skepticism, common in the United States, has been rare in Japan. Implicit in some Japanese views, explicit elsewhere, has been the assumption that room temperature superconductivity is not far away. (Otherwise, even the more optimistic Japanese scientists and engineers see the potential as relatively limited.)

A corollary follows: Japanese executives believe that HTS will be a major battleground for international competition over the next two or three decades.[10] All those interviewed by OTA believed that Japan would have to depend on home-grown technologies in the future. It follows that early exploitation of HTS holds a rare opportunity. Japanese managers—in sharp contrast to their U.S. counterparts—have little doubt that HTS will be a central element in competitive strategies.

Not surprisingly, then, commitments in Japan—as indicated by industrial employees assigned to superconductivity R&D—substantially exceed those here. Box H, based on a survey of Japanese firms conducted for OTA by the U.S. National Science Foundation (NSF), reveals the following contrasts with the United States:

- Although reported budgetary outlays by U.S. industry exceed those in Japan (at $97 million compared with $90 million), Japanese firms reported 900 people working on superconductivity (versus 625 here).
- Total Japanese R&D spending on superconductivity for 1988—industry plus government—should exceed $160 million, U.S. spending $250 million. Such comparisons must be treated with caution, however. The company surveys are incomplete, the fiscal years for the two governments are 6 months out of phase, and the exclusion of some salaries from the Japanese Government budget figures makes the estimate for Japan low by some unknown amount. Finally, spending levels say nothing about the outputs of R&D. Given all these uncertainties, the contrast in numbers of industrial employees takes on the greatest weight.
- Japanese firms are emphasizing prospective applications more heavily. Many companies in Japan are continuing to invest in LTS projects, most of them heavily developmental. They have more engineers assigned to superconductivity R&D than the American firms.

The strength of the Japanese commitment is visible not only in numbers of people, but in the range of businesses represented among the companies that have begun to invest. HTS R&D spans glass and ceramics, shipbuilding and steel, in addition to microelectronics, computers, consumer electronics, and electrical equipment.

The Japanese firms can nonetheless be grouped into two classes:

1. Some have relatively extensive experience in LTS. This group includes manufacturers of superconducting magnets (e.g., for medical imaging systems). A number have been involved in Japan's magnetically levitated train project (see box K later in this chapter). Toshiba, Mitsubishi Electric, and Hitachi have all built and tested prototype LTS generators. Sumitomo Electric, Japan's leading producer of wire and cable, supplied superconducting wire for many of these projects. Sumitomo is also Japan's (and the world's) leader in small synchrotrons—which may emerge as a critical technology for production of next-generation integrated circuits. Finally, a number of Japanese firms have continued to pursue R&D on LTS Josephson devices, with high-performance computers in mind (see box J, later in the chapter).
2. Others, new to superconductivity, began their research programs only after the discoveries in Zurich, Houston, and Tokyo. Some view HTS as important for existing businesses; others seek diversification. The first group includes electrical equipment manufacturers and other suppliers of cap-

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[10] Sixty percent of 167 Japanese companies responding to a mid-1987 survey expected a $20 billion world superconductivity market by 2000—"Superconductor Industry Survey Conducted," *JPRS Report—Science & Technology, Japan*, JPRS-JST-87-068-L, Foreign Broadcast Information Service, Oct. 29, 1987, p. 1 [translated from *Nikkei Sangyo Shimbun*, July 28, 1987]. Electronics applications were ranked most promising, followed by energy storage, and then by a variety of other electric power applications. Nearly 85 percent of those responding to a different survey foresaw applications of room temperature superconductors in industrial equipment by 2010—"Waga kuni ni okeru Gijutsu Kaihatsu no Hoko ni kansuru Chosa" [Survey of Trends in Technology Development in Japan], no. 4, Kagaku Gijutsucho [Science and Technology Agency], 1987, p. 12.

Box H.—Superconductivity R&D in Japanese Companies

NSF's Tokyo Office conducted a mail survey of 43 Japanese companies for OTA in early 1988, receiving responses from 38 firms with significant HTS R&D activities. As with the U.S. survey, the sample was intended to cover Japanese firms with substantial activity in HTS, but it was not exhaustive. Indeed, undercounting of people and R&D funding in the Japanese survey almost certainly exceeds that for the U.S. survey. Not only did the U.S. survey cover more companies, but the list of Japanese firms that have joined MITI's International Superconductivity Technology Center (ISTEC) shows many participants that the Japan survey did not reach.[1]

Level of Commitment

- As of early 1988, the 38 firms surveyed had approximately 900 scientists and technicians at work on superconductivity R&D (both LTS and HTS), either part-time or full-time.
- In total, the 38 firms expected to spend about $90 million on superconductivity R&D in 1988.
- Seventeen of the firms reported a current annual commitment of $1 million or more for superconductivity R&D, with 3 spending in the $1 million to $2 million range, 8 at $2 million to $5 million, and 5 putting between $5 million and $10 million into superconductivity R&D; one firm is investing more than $10 million.

Types of Firms

- The 38 firms fall into the following broad categories: primary metals, and wire and cable (8); glass, chemicals, and specialty materials (12); electrical machinery and equipment, and electronics, including telecommunications (15); and other (3).
- The 1987 revenues of the companies ranged from $72 million to $46 billion, with a median of $3.2 billion. R&D spending ranged from 1 percent of sales to 12 percent; the median is 3.2 percent.
- Average 1988 R&D spending on superconductivity in the four categories is as follows:

primary metals, wire & cable	$4.0 million
electrical machinery and electronics	$3.3 million
glass, chemicals, specialty materials	$0.8 million
other	$0.3 million

R&D Activities

- Of the 900 people engaged in superconductivity R&D, three-quarters, as in the United States, are scientists/engineers and one-quarter are technicians. The scientists/engineers divided roughly as follows:

materials scientists	34 percent
physicists	28 percent
electrical engineers	20 percent
chemists	18 percent

- All 38 firms reported some work underway on materials characterization and processing. Fifteen of the 17 firms spending at $1 million and up have projects directed at electronics applications of HTS, and half are working on high-current, high-field applications. Most of the companies with smaller HTS efforts are focusing more on materials R&D and processing than on applications.
- Ongoing LTS work seems markedly greater than among the U.S. firms. Among the companies spending $1 million or more on superconductivity R&D, 11 have LTS projects underway, and many expect to direct roughly equal resources to LTS and HTS.

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[1] By May 1988, 45 Japanese companies had joined ISTEC as full members—Report Memorandum #155, Tokyo Office of the U.S. National Science Foundation, May 13, 1988. The survey reached only 25 of these, and an even smaller fraction (5 of 45) of associate members. For some of these companies, of course, superconductivity may be a new area of R&D, with little internal activity.

All currency conversions in this box, as elsewhere in the chapter, have been made at 130 yen to the dollar.

Other Observations

- About two-thirds of the Japanese firms reported some form of cooperative R&D in HTS. One-third expect to engage in cooperative R&D internationally in the future; two firms are currently working with American companies.
- Perceptions of where "the most significant HTS R&D is currently being done" centered on Sumitomo Electric, Japan's National Research Institute for Metals, Tokyo University, IBM, and AT&T. When asked which country was ahead, 13 responded that the United States had the lead in HTS R&D, 7 indicated Japan, and another 10 rated the two countries even (the remaining 8 did not answer this question).
- Ten of the seventeen companies spending $1 million or more have received government funding for HTS R&D.

ital goods. It also includes steelmakers, who have begun speculating, for instance, that magnetic levitation of strand-cast products could lead to better surface quality and higher yields (box I). The steelmakers are also trying to diversify, along with glass companies and shipbuilders. All these industries are in decline; opportunities for diversification and continued growth hold great attractions in Japan.

Regardless of industry, many Japanese firms are pursuing research and applications development in parallel fashion.[11] They have basic work underway, mostly in characterization, and people searching for materials with better properties. Development projects include work on thin films and efforts to fabricate wires. In these activities, the Japanese are proceeding much like their counterparts at the leading U.S. industrial laboratories.

Japanese efforts differ in one major respect from those in the United States. Many firms in Japan have groups at work on feasibility studies and exploratory "what if" exercises. (Government programs, treated later in the chapter, show the same thrust.) These groups have a specific task: to think about possible commercial applications. In some cases, the efforts have already been carried to the stage of preliminary designs and marketing analyses. The work is highly speculative, of course, but the Japanese believe it will help prepare for commercialization. Only a few U.S. companies have begun similar efforts.

U.S. and Japanese Strategies Compared

The Japanese see applications coming relatively quickly. When queried in the spring of 1987, scientists and research managers in Japan called for more basic research in their country, and efforts to develop applications based on patents filed in the United States.[12] Like Americans, they viewed superconductivity as largely a research enterprise for now—with the research laying groundwork for commercial competition that would come soon, perhaps as soon as one to three years. In essence, Japanese companies are pursuing a three-pronged R&D strategy: 1) basic research; 2) development, aimed mostly at processing; and 3) product planning and market evaluation. The last of these carries the gravest potential consequences for U.S.-Japan competition. *If technical developments in HTS proceed as rapidly over the next two or three years as during 1987, Japanese firms will be in better positions to move toward commercial applications than American companies.*

If U.S. firms wait to think about product and process developments until the research results

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[11] As Sumitomo Electric Vice President Nakahara explains it, Japanese companies and other research organizations should pursue basic research and applications on "parallel tracks" to ensure cross-fertilization of efforts—*Chodendo to wa Nanka* [What is Superconductivity], Nihon Keizai Shimbunsha, 1987, p. 91.

[12] "Chodendo Busshitsu: Nichibei Gokaku no Kaihatsu Kyoso" [Superconducting Materials: Japan and the U.S. on a Par in Competition for Development], *Nikkei Sangyo Shimbun*, May 12, 1987.

Box I.—HTS R&D at Nippon Steel

Why did the largest steel producer in the world put 40 people to work on HTS in February 1987? Nippon Steel may be the leader in market share, but its sales have been declining—partly the result of structural changes in the Japanese economy, leading to declining demand for steel, and partly a consequence of greater competition in international markets. Company strategists have two major tasks: 1) finding ways to make steel more cheaply, thereby helping the company compete in its primary if shrinking businesses; and 2) identifying new opportunities. Superconductivity fits both objectives.

Planners see Nippon Steel as bringing three primary technical strengths to HTS. First, the company has always designed much of its own production equipment. It has process engineering skills, not only for making steel, but for titanium and other metals as well. Second, the firm has expertise in wire manufacture—a technology that could turn out to be important as HTS matures. Finally—and most important—Nippon Steel has worked hard over the years to develop technical capabilities in ceramics. Originally, most of this work was in refractories for furnaces. More recently, the company has sought to diversify into high-technology ceramics, and also into silicon production for the semiconductor industry. To the extent that the new superconducting materials will demand expertise in ceramics and other advanced materials, Nippon Steel believes it will be well-placed.

None of these perceived strengths may turn out to be sufficient to place the firm in the forefront of HTS. Nippon Steel's executives might be grasping at straws. Nonetheless, the company has looked with some care at 50 or more potential applications of HTS. Some of these analyses have been taken to the point of comprehensive feasibility studies. For example, company engineers have evaluated the prospects for continuous strand casting using superconducting magnets to confine and float molten steel, followed by in-plant materials handling also based on magnetic levitation.

are in they may lose out competitively. Japanese companies will already have thought through those steps, weighed the potential problems, considered alternatives—perhaps anticipated some of the follow-on technical work and even begun to pursue it. The Japanese approach probably costs more—some R&D groups will pursue false trails, companies may be paying the salaries of too many people working on overlapping projects—but the eventual rewards could more than make up for this. Japanese managers find it strange that American companies believe they can track a technology's development, waiting for the right time to begin product development, without actively and aggressively pursuing that technology in their own laboratories. (Of course, many Japanese firms did just this not so many years ago.)

JAPAN'S HTS INITIATIVES

Many countries are pursuing HTS research, but talk of a superconductivity race has focused on the United States and Japan.[13] Sumitomo's many hundreds of patent applications, for example, have drawn widespread attention. The race is certainly a real one in terms of science. Laboratories around the world confirmed superconducting behavior in the thalium-based materials in a matter of hours.

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[13] See, for example, "Two Different Cadences in the Superconductor Race," *Washington Post*, May 20, 1987, p. A1; "U.S. 'Leading Slightly' in Superconductor Race," *Japan Economic Journal*, June 13, 1987, p. 15.

This section is based in part on interviews with government officials in Japan during the fall of 1987. For background on Japanese industrial policy, see *International Competitiveness in Electronics* (Washington, DC: Office of Technology Assessment, November 1983), pp. 413-422.

On patenting, below, see S.K. Yoder, "Rush to Exploit New Superconductors Makes Japan Even More Patent-Crazy," *Wall Street Journal*, Aug. 27, 1987, p. 1

Preoccupation with Japanese efforts is hardly a surprise, given Japan's huge trade surplus with the United States, and the longstanding view by some that Japanese companies have been getting a free ride from American research.[14] Press reports have suggested that Japan is taking off in superconductivity, with government agencies in the lead.[15]

MITI asked for 3.5 billion yen (about $27 million at 130 yen to the dollar) for superconductivity in fiscal 1988, well above the previous year's level (550 million yen, $4.2 million). (Japanese budget figures do not include breakdowns for high- and low-temperature superconductivity.) But the image of a government-coordinated, crash program in HTS is false. The policy environment for superconductivity remained in flux in Japan during 1987. The difference is this. After the middle of the year, the outlines of Japanese Government policies began to solidify. By early 1988, they had taken shape. U.S. policies, in contrast, remain in a state of considerable disarray.

Government Resources for Superconductivity

In Japan, four principal agencies have competed with one another for resources to support HTS (table 6): the Science and Technology Agency (STA); MITI; the Ministry of Education (Monbusho); and the Ministry of Transport. Key players in government, industry, and universities have been seeking to get into the superconductivity game, looking for money and the authority to expand their programs.

Table 6.—Major Japanese Government Programs and Activities in Superconductivity

Science and Technology Agency (STA):
- *Multicore Project*
  Nine laboratories and other government organizations participating in work on theory and database development, materials characterization, processing, and technology transfer. The lead laboratories are the National Research Institute for Metals (NRIM, with work on theory, databases, thin films, and a superconducting generator) and the National Institute for Research on Inorganic Materials (materials synthesis and new material development, crystal structure determination, microstructure control).
- *Japan Atomic Energy Research Institute*
  R&D on superconducting magnets and applications.
- *Japan Research Development Corporation*
  Primarily measurement work.
- *New Superconductivity Materials Research Association (Forum)*
  Primarily information exchange.

Ministry of International Trade and Industry (MITI):
- *Electrotechnical Laboratory (ETL), plus other MITI facilities*
  R&D on superconducting electronics (e.g., Josephson devices), as well as new materials (including superconducting polymers).
- *Moonlight Project*
  Superconducting generator.
- *Support for Technologies Needed for Research and Processing*
  Thin film fabrication techniques, low temperature processing of bulk materials.
- *Research Associations*
  (See text.)
- *International Superconductivity Technology Center (ISTEC)*

Ministry of Education (Monbusho):
- *University research support*
  Examples: Professor Tanaka's group at Tokyo University, working particularly on new materials; Professor Muto's work on theory at Tohoku University; support for the Ceramics Center at Tokyo Kogyo University.

Ministry of Transportation:
- *Support for the Magnetically Levitated Train project at the Railway Technical Institute*

SOURCE: Office of Technology Assessment, 1988.

Much of the substance of Japanese industrial and technology policies emerges from behind the scenes, the product of long-standing ties among government officials, corporate executives, and, in a case like HTS, senior professors in the leading universities. A host of advisory committees, reports, and budget proposals also contributed to the rise of superconductivity on Japan's policy agenda. The statements of the Council for Science and Technology—an advisory group chaired by the Prime Minister and including the directors of MITI, STA, and other major ministries—are typical. In August 1987 the Council issued a report on superconductivity calling for "hybrid" basic research, involving experts from many fields. Japan's national laboratories should promote cooperation with industry and universities, and reward individual excellence, the Council urged, envisioning HTS R&D as a major step in improving Japan's creative research capabilities.

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[14] This perception is an exaggeration. See *International Competition in Services*, op. cit., pp. 202-203.

[15] For example, "On one hand, companies vie to beat each other to the patent office and marketplace. But at the same time, arch-rivals join forces on certain tasks when speed is essential and the research is risky. And the MITI orchestrates it all." See S.K. Yoder, "Superconductivity Race Shows How Japan Inc. Works," *Wall Street Journal*, Aug. 12, 1987, p. 6. The same article calls superconductivity a Japanese "obsession."

STA and the Multicore Project

Each agency is independently pursuing these broad goals. In the summer of 1987, when HTS became the focus of attention worldwide, STA set up a Committee for the Promotion of Research and Development of Superconducting Materials. The nine members represent the major STA laboratories, the Japan Research Development Corporation, and the Interministerial R&D Division of the STA Research and Development Bureau. The Committee's report stressed the many unknowns concerning superconductivity, and recommended a high priority for basic research—not surprising, given the STA mission.[16]

Highlighting the role of government, particularly STA, the Committee urged that Japan's national laboratories, already credited with significant contributions in HTS, accelerate their efforts even more. While emphasizing the need for research, the members also advocated preparations for commercialization of new products. Their report touches on opportunities for international cooperation, recommending that Japan's joint government-industry R&D programs be opened to foreign participation, and that Japan seek to make a global contribution to HTS. At the same time, the Committee underscored the importance of making research results from STA laboratories—of which there are five—widely available.

The STA Committee sees these laboratories, and the agency's new Multicore Project, as the bridge between university science and corporate applications. Companies and universities participate through the New Superconductivity Materials Research Association, which had about 130 members at the end of 1987. The Multicore Project, with a budget of more than 2 billion yen (about $16 million) for fiscal 1988, aims to strengthen the capabilities of STA laboratories in HTS, and to speed transfer of research results to industry.[17]

A number of STA laboratories will get more money for HTS—notably the National Research Institute for Metals (NRIM), which plans a major thrust in materials characterization. NRIM scientists discovered the bismuth oxide HTS composition in January 1988. The National Institute for Research on Inorganic Materials and the Atomic Energy Research Institute will get most of the rest of the STA money. Nine laboratories in total will participate in the Multicore Project, with "core" research work going on in each.

Given this decentralized approach (no physical relocations are planned), STA will rely on a steering committee with representatives from industry and universities, as well as government, for coordination. The steering committee's job will be a difficult one, the more so if—as STA officials hope—MITI laboratories can also be pulled into the Multicore Project. At present, however, MITI has its own quite independent plans.

MITI Programs

A recent report by the Advisory Committee on Superconductivity Industrial Technology Development, made up of representatives from industry and universities, reflects the perspective of MITI—to Western eyes, the most visible agent of Japanese industrial policy.[18] Like STA, the MITI committee sees the government role as one of helping industry make use of research results from the national laboratories and universities. But MITI goes further in advocating national projects, not only for R&D on the new materials, but for applications in electronics and electrical machinery. Box J notes the Ministry's support for Josephson computing technologies—a field where Japan began by following the path laid down by IBM and other U.S. companies, then persisted after American firms cut back their efforts.

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[16] "New Developments in Superconducting Materials R&D," Science and Technology Agency, Tokyo, Sept. 21, 1987.

[17] *Chodendo Zairyo Kenkyu Maruchikoa Projekuto 63 nendo Kisan Yokyu Sokatsuhyo* [Budget Request for Multicore Project for FY1988]; also "Superconductor R&D to Industrial Application," *JPRS Report—Science & Technology, Japan*, JPRS-JST-88-007-L, Foreign Broadcast Information Service, Mar. 11, 1988, p. 100 [translated from *Nikkan Kogyo Shimbun*, Jan. 1, 1988]. The Multicore name signifies that multiple organizations form the core of the project, emphasizing the thrust toward coordination and reorientation rather than an all-new initiative. The project accounts for about two-thirds of STA's fiscal 1988 budget request for superconductivity, which totals 3.1 billion yen (Japan's fiscal year begins in April.) STA has also sought funding for a superconducting generator project.

[18] *Chodendo Sangyo Gijutsu Kaihatsu Kondankai* (1988).

A good portion of MITI's 1988 superconductivity budget will go toward applications. Examples include a new project on thin films and Josephson devices, part of the "Technologies for Next Generation Industries" program of the Agency for Industrial Science and Technology (AIST is part of MITI). The Ministry's 70 megawatt (MW) generator project, based on LTS technology and also scheduled for more than $10 million—a hefty slice of the 1988 MITI superconductivity budget—follows several years of feasibility studies. Motivated in part by the search for energy savings, goals for the 8-year project range from improvements in methods for processing superconducting wire to construction of a complete prototype.[19] Officials say that HTS technologies will be utilized if available.

Late 1987 saw a major step for the 70 MW generator project, the formation of a research association (*kenkyu kumiai*). As is typical of these research associations—central mechanisms of Japanese technology policy—MITI not only helped with the planning, but will assist with administration and furnish ongoing financial support. Likewise typical of MITI projects, the research association brings together participants with a range of technical strengths, and companies from different industries: the members include two cryogenic engineering firms, the Fine Ceramics Center, the Central Electric Power Research Institute, and a number of major electric power and electronics firms. MITI sees its role as supporting industry not only by creating incentives for applications-related R&D, but by spurring productive interactions among firms and industries that might not otherwise collaborate.

MITI, like STA, also runs its own laboratories. The Electrotechnical Laboratory—which has won worldwide respect for its research—has been involved in superconductivity since the middle 1960s, when the laboratory began R&D on LTS magnets for MHD (magnetohydrodynamic) power generation under the Moonlight Project. More recently, ETL has attracted particular notice for its work on niobium-based Josephson devices (box J). ETL's overall 1987 budget came to $57 million; like many organizations in the United States, the laboratory was able to reprogram funds internally for HTS during 1987; MITI will get $2.5 million for ETL research on the new superconductors in 1988. The laboratory has several groups, and about 40 people in total, working on superconductivity (the ETL research staff numbers 560).

The Ministry seeks to involve private corporations in its efforts through mechanisms ranging from research associations to advisory boards and symposia. Industry is MITI's major constituent, and the Ministry's HTS programs will follow patterns laid down over the years for supporting other industries and other technologies—e.g., semiconductors, computers, biotechnology.

The Ministry of Education

The Monbusho, which supports university research, has a larger R&D budget than any other arm of the Japanese Government. Sup-

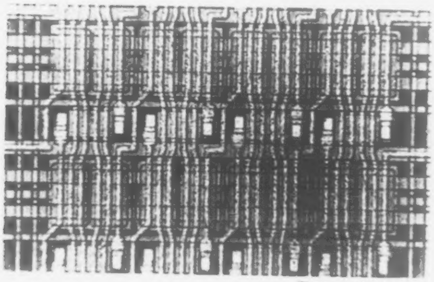

Photo credit: IBM Corp

Memory cells in experimental Josephson junction integrated circuit chip

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[19] *Chodendo Hatsuden Kanren Kiki-Zairyo Gijutsu no Fizabirite Chosa Kenkyu*, March 1987. The original proposal, advanced in 1985, was much more ambitious, calling for a 200 MW generator to be built in 5 years.

Box J.—Josephson Junction Computer R&D: From the United States to Japan[1]

The pursuit of a Josephson-based computer has taken quite different paths in the United States and Japan since the early 1970s. Josephson devices provide the fastest electronic switches known, hence—in principle—the fastest digital computers. Because they are superconducting devices, with very little power dissipation, JJs can be packed tightly together. Theoretically, therefore, a computer built with JJs could be very compact, as well as extraordinarily fast and powerful.

U.S. Efforts

Three U.S. corporations pursued JJ R&D for computer applications: AT&T, IBM, and Sperry Univac (which later merged with Burroughs to form Unisys). Each made significant contributions to the JJ technology base. Beginning in the 1960s, more than 10 years of research at AT&T's Bell Laboratories produced a much better understanding of the physics of JJs. IBM went much farther, building a prototype of the circuitry for a complete computer, as well as exploring fabrication methods for JJ logic and memory chips. Sperry concentrated on JJs made from refractory materials such as niobium and niobium nitride (instead of the lead alloy used by IBM), and developed processing methods for high-performance, all-niobium circuits.

All three companies had scaled back or abandoned their JJ projects by the early 1980s—each for its own reasons. AT&T terminated the Bell Laboratories program in 1979 after deciding that the technical hurdles to practical applications were formidable. Sperry abandoned its effort to develop a JJ computer in 1983, after closing its Sudbury, Massachusetts, research center, the focus of the work. (JJ research by Sperry's Defense Systems Division, aimed at sensors, continued.)

IBM, with the most ambitious program, was spending about $20 million annually by the early 1980s, with the National Security Agency (NSA) providing about $5 million of this. Although NSA urged continuation, IBM drastically scaled back its effort in 1983, ending pursuit of a working computer, after its Yorktown Heights Laboratory was reorganized and the JJ work came under new management. Logic chips based on IBM's experimental production technology performed adequately, but the memory did not; the new management team estimated that improving the memory chips would add another 2 years to the schedule. By that time, management reasoned, continuing progress with more conventional silicon and/or gallium arsenide chips would make it hard for a JJ-based machine to offer compelling advantages in speed or processing power.

Before ending its JJ program, IBM came close to an agreement with Sperry for joint development of Josephson technologies. IBM had the most advanced designs but was struggling to fabricate them, while Sperry had proven processing technologies. The agreement was almost 18 months in the making, and had apparently cleared the antitrust hurdle after the NSA proposed taking the project under its wing. But the agreement was never consummated because Sperry's management decided to decentralize its R&D among its operating divisions, and reassigned its JJ computer group to the Defense Systems Division—a reassignment that key technical employees declined.

Japan's Program

Josephson junction R&D in Japan seems to have been driven by national as much as company interests. Spurred by IBM's advances, MITI's Electrotechnical Laboratory (ETL) initiated JJ research in the middle 1970s. Nippon Telephone & Telegraph (NTT) also launched a JJ program—now one of the biggest in the world, with 40 professionals working on device design and fabrication. In 1980, MITI established a supercomputer project with a 10-year budget of $100 million and the goal of building a 10-gigaflop machine by 1990. ETL's ongoing effort was absorbed into this new initiative, and expanded, with MITI supporting JJ work at Fujitsu, NEC, and Hitachi.

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[1] Much of the information in this box is based on interviews. Also see A.L. Robinson, "New Superconductors for a Supercomputer," *Science*, Jan. 1, 1982, p. 40; A.L. Robinson, "IBM Drops Superconducting Computer Project," *Science*, Nov. 4, 1983, p. 492; "JTech Panel Report on Opto- & Microelectronics," Japanese Technology Evaluation Program, Science Applications International Corp. under contract No. TA-83-SAC-02254 from the U.S. Department of Commerce, May 1985, Section 11; "Government's Role in Computers and Superconductors," prepared for OTA by K. Flamm under contract No. H3-6470, March 1988, pp. 47-52.

The Japanese clearly benefited from U.S. R&D on Josephson devices. Bell Laboratories scientists published their findings widely and exchanged information on a regular basis with colleagues in the United States and Japan. During the 8 years of Sperry's active involvement in research on JJ-based computers, the company's leading researchers visited Japan several times—at MITI's expense.

Sperry and IBM technology laid the groundwork for nearly all the Japanese developments in JJ computing. Fujitsu's technology, for example, has been based almost entirely on Sperry's original designs and fabrication methods. More recently, U.S. visitors to Japan have been impressed with the advances in JJ logic and memory circuits emerging from Japanese laboratories, especially with the work on manufacturing techniques.

Lessons

American firms made a good deal of technical progress on JJ-based computers before largely abandoning the field. The Japanese continued, in part because of MITI's push, and they too have advanced the technology. It is too early to say whether the Japanese work will eventually yield a practical computer based on Josephson electronics. But it has become clear that, technologically, the Japanese have made considerable headway.

Fujitsu and NEC have demonstrated LSI (large-scale integration) chips containing thousands of JJs. Both firms are within reach of a simple computer based on a 16-bit JJ microprocessor, and seem likely to reach this goal by their 1990 target dates. Like IBM earlier, the Japanese may find that the margin of improvement will not be enough to compete with silicon (or gallium arsenide). In other words, the Japanese could find themselves with a technical success but a commercial failure.

And certainly it is too early to tell whether Japan will have an edge over the United States in developing HTS JJ electronic devices as a result of its persistence with LTS JJs. Indeed, some believe that HTS JJ devices will never prove technologically useful. Four points with potential relevance for HTS nonetheless emerge from the JJ computer experience:

1. Long-term R&D becomes hard to justify when viewed only in terms of costs: Sperry's research center became "too expensive," contributing to the loss of the U.S. lead in a potentially significant technology.
2. Diverse approaches to R&D, as in the three U.S. companies, increase the likelihood of eventual technical success.
3. Complex and demanding technical goals call for a parallel (rather than serial) approach to R&D, with feedback and cross-fertilization among complementary streams of research. In Japan, for example, the Monbusho supported research on materials problems, supplementing MITI's efforts on device physics and the architecture and system design aspects of a Josephson computer.
4. Concrete goals—as in MITI's supercomputer effort—can be an important motivating force for R&D, as well as helping to define intermediate research objectives.

port for superconductivity is relatively new, however, going back only to 1984. In 1987, Monbusho funded 41 mostly small projects on superconductivity at Japanese universities, with spending totaling $4.3 million.[20] For fiscal 1988, Monbusho spending on superconductivity will reach about $14 million. Although the Education Ministry has placed superconductivity at the top of its list for greater support, it ranks third behind MITI and STA in its 1988 budget for direct support of superconductivity.

In addition to the many small projects it funds, Monbusho provides much of the support for a few large programs headed by internationally known scientists. For instance, Professor Shoji Tanaka's group at Tokyo University will receive more than $700,000 during 1988. Professor Tanaka (who recently retired from the university to direct research at MITI's International Superconductivity Technology Center, ISTEC), has also been awarded a 3-year grant of $1.6 million for "Specially Promoted Distinguished Research." Professor Yoshio Muto, at Tohoku University, whose group has been designated one of 30 priority research projects, will get more than $2 million over the 3-year period 1988-1990. These large university-based efforts generally include participants from a number of universities, selected by the lead professor.

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[20] *Daigaku Kankei ni okeru Chodendo Kenkyu no Tsuishin ni tsuite* [Concerning Support for Superconductivity Research in Universities], Monbusho. The Ministry of Education's figures for support of university R&D normally exclude salaries, which are paid out of other accounts. (Other Japanese Government agencies typically include salaries in their published R&D budget figures, just as in the United States.)

Taking university research as a whole, the scope in Japan is narrower than in the United States, and the quality substantially lower. Japan has fewer centers of excellence, and a more rapid drop-off in quality as one moves down the scale. The best institutions in Japan are very good. There simply are not that many of them.

Superconductivity, however, has been an exception. Before the discoveries in HTS, the field had been something of a backwater. Interest had been declining in the United States, more so than in Japanese universities. Recently, American scientists have given the Japanese high marks for research in superconductivity.

Tokyo, Tohoku, and Kyoto Universities have been getting about three-quarters of Monbusho superconductivity funding. Some of the research groups at these schools—e.g., Tokyo University's in superconductivity—are on a par with the best in the world. And even in their less known schools, the Japanese excel at some kinds of work—notably painstaking empirical research. Most important, R&D in Japan's universities is improving rapidly, in part because of the efforts of the younger faculty members trained in the United States.

The Ministry of Transport and the Maglev Train

Japan's magnetically levitated (maglev) train project—box K—which has been underway for two decades, is scheduled to get more than $4 million during 1988 from the Ministry of Transport, which oversees the effort. While current prototypes use LTS magnet systems for both suspension and propulsion—and a relatively small fraction of the program's funds go toward superconductivity R&D—the engineers leading the project hope that HTS materials can eventually be incorporated.

The maglev program typifies the kind of long-term, continuing effort—in this case beginning in the 1960s—that Japanese decisionmakers expect will pay off in eventual commercialization. Although maglev R&D supported by the U.S. Government ended in 1975, the Japanese have persevered. To Japan, the linear motor car has become a symbol of indigenous technology development.

Summarizing the Government Role

As the many different programs mentioned above suggest, the scene has changed rapidly in Japan. Major ministries involved in superconductivity R&D steered more money to HTS during 1987, and have substantially higher budgets for 1988. A superconductivity city has even been proposed, where research would be centralized and applications tested.[21] Increases in government spending send unmistakable signals to industry, as well as to the universities and national laboratories.

Japanese industrial policy works primarily through incentives. Ministries seek advice from

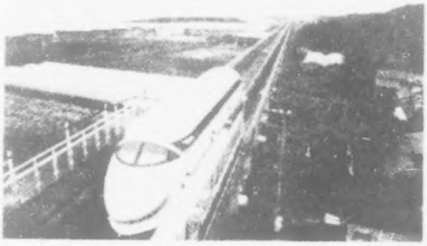

Photo credit: Japan External Trade Organization

Japan's prototype linear motor car, a magnetically levitated train

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[21] No agency has linked itself with the proposal, which seems to be a trial balloon—"Superconductivity City Project," *Science and Technology in Japan*, November-December, 1987, p. 43.

Box K.—Japan's Magnetically Levitated Train Program[1]

In 1979, a prototype Japanese train without any people aboard set a (then) world record speed of 321 mph. In the spring of 1987, a later version—able to carry 44 passengers—began demonstration runs. This well-known high-speed train project (sometimes called the linear motor car) can be traced back to studies of linear induction motors for high-speed propulsion at Japan National Railways (JNR) during the 1960s. The maglev concept itself—the use of superconducting magnets to float a train—apparently originated in the United States, and first got heavy publicity during the late 1960s. A few years later, JNR began operational tests on a superconducting maglev motor car at its facilities on the southern island of Kyushu. The prototypes use onboard magnets, wound with niobium-titanium alloy and cooled with liquid helium. Participating companies—including Toshiba, Mitsubishi Electric, and Sumitomo Heavy Industries—have gained a good deal of valuable experience in LTS engineering from their design and fabrication of these systems.

The program demonstrates the continuity that characterizes both public and private technology development efforts in Japan. The Japanese have spent roughly $100 million in bringing the technology to its current stage. Since the privatization of JNR in 1987, work has continued at the Railway Technical Institute, now jointly owned by seven newly private railroads. To policymakers in Japan, application of HTS to the linear motor car seems a potentially attractive vehicle for pushing the technology forward and demonstrating its usefulness.

Like the bullet train before it, the intent of the maglev project has been to keep Japan in the forefront of railroad technology while finding practical solutions to transportation problems in a crowded and cramped country. The bullet train can reach 150 mph; the linear motor car aims to double that. Only a West German project, using normal magnets in a different configuration, comes close in terms of its potential to radically alter rail transportation. The aim in both countries is to break through the limitations of wheel-on-rail technology in terms of speed, noise and vibration, and maintenance costs (which go up rapidly with speed because of high dynamic forces and the need for very precise track alignment). Maglev offers potential for moving people over distances of several hundred miles with travel times comparable to air service, and lower energy consumption. The 310-mile trip from Tokyo to Osaka—the most heavily traveled route in Japan—currently takes about 2⅓ hours on the bullet train. The Japanese hope to cut this to a bit over an hour.

When the maglev effort began, Japan had no guarantees that a levitated train would be technically or economically feasible. JNR had identified a major future problem—very heavy traffic on existing rail routes, with the need for faster trains and greater carrying capacity—and sought solutions. The corporation, then publicly owned, gained approval for a long-term research effort—a project of a scale and duration that would be highly unusual in the United States (outside of defense and space).

The project also illustrates the way in which applications drive technical development at more fundamental levels. Superconducting magnets had never before been designed and built for such a purpose—levitation of a train. Choosing superconducting (rather than normal) magnets, in turn, forced solutions to problems of magnet design and fabrication, as well as handling liquid helium in a rather difficult environment. Because Japan imports all its helium, the designers chose a closed-cycle refrigeration system (based on Western technology), so that no gas would be lost. This led ultimately to new cryogenic (very low temperature) refrigeration technology. Successful development of the refrigeration equipment—not an easy task—provided an experience base that has already proved useful in other low-temperature technologies, and could well help with some HTS applications. About 20 of these refrigeration systems have been manufactured for sale.

The Railway Technical Institute—showing more than a touch of optimism—hopes to have levitated trains running on scheduled routes before the turn of the century, and claims costs will be lower than those of the bullet train. With Diet members expressing support for construction and advocating alternative routes, debate has become intense on issues ranging from cost to environmental impacts. The Minister of Transportation has announced plans for a 2-year feasibility study to begin in 1988, followed by construction of two maglev routes starting in 1990. Japan would like to sell this technology to the United States.

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[1] This box is based in part on interviews. Also see Y. Kyotani, "Jiki Fujo Rineaka Kaihatsu no Omoide," [Remembrances Concerning the Development of Maglev], *Unteo Kyokaishi*, series beginning in January 1987; "Rineaka Jitsuyozen ni Kangaeru beki" [What We Ought to Think about Before Commercializing Maglev], editorial, *Nihon Keizai Shimbun*, Sept. 8, 1987, p. 2; W. Sullivan, "Race for the Fastest Train: Japan Builds a New Prototype," *New York Times*, Sept. 15, 1987, p. C1; "Floating Trains: Linear-Motor Cars Ready to Beat the Bullet Train," *Nikkei High Tech Report*, Nov. 9, 1987, p. 1; "Status of Magnetically Levitated Vehicle (Maglev) Development," *JPRS Report—Science & Technology, Japan*, JPRS-JST-87-031, Foreign Broadcast Information Service, Dec. 2, 1987, p. 81 [translated from *Jidosha Gijutsu*]; "Unyosho ga Iko Hyomei" [Ministry of Transport Demonstrates Intent], *Nihon Keizai Shimbun*, Jan. 22, 1988.

On the technology, see *U.S. Passenger Rail Technologies* (Washington, DC: Office of Technology Assessment, December 1983), ch. 6; also *Maglev Transport: Now and for the Future* (London: Institution of Mechanical Engineers, 1984). On the origins of the maglev concept, see J.R. Powell and G.R. Danby, "A 300-mph magnetically suspended train," *Mechanical Engineering*, November 1967, p. 30.

business leaders in the early stages of policy development. The processes through which officials in government and the private sector interact, and informal encouragement of industry efforts by government, arguably play a role at least as important as direct financial support. Government funding for R&D projects tends to be modest; consistently, private industry has paid for three-quarters or more of all Japanese R&D, compared with about half in the United States (table 5).

At the same time, public funds for the maglev train provided a stimulus for companies like Sumitomo Heavy Industries to build their experience base in superconductivity and related problems in cryogenic engineering. Japanese companies participate as members of consortia formed by MITI to undertake projects such as the 70 MW generator. In the Japanese view, such projects leverage public investment, helping break technological bottlenecks and diffuse results to industry. Companies participating in Japan's government-sponsored R&D efforts normally contribute about half of the project funding.

When it comes to basic research, the government share is about 50 percent in Japan, versus two-thirds here. As pointed out earlier in the chapter, many leaders in business, government, and universities in Japan are pushing for improvements in basic research, seeking greater creativity and originality. Because of budget pressures, Government support for basic science has been growing at an annual rate of only 3 percent, slower than the overall rate of R&D growth in Japan. Thus the government share of basic research funding has been declining.

As the yen rises relative to the dollar, Japanese spending, when translated into dollars, appears more impressive. Calculated at exchange rates current at the end of 1987, direct Japanese Government support for superconductivity—exclusive of salaries—seems likely to be about $70 million during 1988. Budget figures in Japan do not break out LTS and HTS, but a good portion of the total will no doubt support ongoing work with low-temperature materials. Funding increases have been sharp, coming after a period of relatively low spending on superconductivity (leaving aside such projects as the linear motor car, where superconductivity is a means to an end). In fiscal 1986, for example, MITI spent about $2 million on LTS technologies. And set against overall Japanese Government R&D support—itself relatively small compared to corporate R&D—superconductivity remains a minor item.[22]

MITI's 1988 HTS budget exceeds that of the other agencies, but it would be a mistake to conclude that MITI is tightly coordinating Japan's superconductivity policies. In OTA's interviews, MITI officials argued that, at this stage in the development of HTS, competition among ministries and research groups should be seen as healthy. STA staff, meanwhile, hopes that MITI laboratories will eventually join the Multicore Project—while conceding that this is unlikely in the near term.

Who Has the Lead Role, Government or Industry?

Westerners often misconstrue relationships between government and industry in Japan. MITI and other ministries may try to influence corporate decisions, but Japan's Government does not issue directives to industry. A more accurate picture of Japanese policymaking sees government-industry interactions based on processes of "reciprocal consent"—continuing discussion and negotiation.[23] Corporate leaders are heavily involved in building consensus and helping shape government programs. HTS will be no exception.

In superconductivity, industry has influenced government policies through frequent meetings with ministry officials. At least a third of the members of MITI's Advisory Committee on Superconductivity Industrial Technology Development come from the private sector. More than a hundred Japanese corporations belong to the STA's newly formed *Shin Chodendo*

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[22] The Japanese Government budget for all science and technology activities totals 1,700 billion yen for fiscal 1988—about $13 billion. See Report Memorandum #147, Tokyo Office of the U.S. National Science Foundation, Feb. 5, 1988.

[23] R.J. Samuels, *The Business of the Japanese State* (Ithaca, NY: Cornell University Press, 1987).

Zairyo Kenkyukai [New Superconductivity Materials Research Association], best known as the superconductivity forum.

The forum, chaired by Dr. Shinroku Saito, serves as a "window" between corporate members (who pay an annual fee of about $1,000) and the universities and national laboratories involved in the Multicore Project. According to the director of STA's Research and Development Bureau, the forum will hold workshops and symposia, undertake "brainstorming" in support of the Multicore Project, and encourage cooperation in research, both domestically and internationally. Many participants, including Dr. Saito, also advise other ministries; thus the forum helps build linkages within the Japanese Government.

The Fine Ceramics Center (FCC) illustrates a different mechanism. Government and industry have both provided money for an extraordinarily well-equipped laboratory in Nagoya, with participating companies sending scientists and engineers. In contrast to some other MITI-sponsored R&D efforts, many of which have had staffs viewed as second rate, the FCC appears to have attracted highly qualified people. The companies continue to pay their salaries, and they help transfer technology from the Nagoya laboratory back to their employer.

When it comes to Japan's national laboratories, opinions differ as to whether corporations give more than they receive. For instance, at the National Research Institute for Metals, which normally hosts a half-dozen people from industry who work alongside NRIM scientists, laboratory officials contend that they have been ahead of industry in at least some areas of superconductivity. Organizations like NRIM also let contracts to companies, including large corporations (New Japan Steel, Toshiba). While laboratory managers view contract research as a mechanism for helping industry, the companies —which make little profit on such work—tend to see it as part of their contribution to the larger national effort.

In the future, government scientists may have a chance to spend time working in corporate laboratories—some of them much better equipped than government facilities—but so far this has been rare. Legal provisions, only recently relaxed through new legislation, have limited such arrangements.

Direct cooperation in research between companies and universities has likewise been limited. This is also changing, however. Professor Tanaka recently had 10 scientists from a group of private companies working in his Tokyo University laboratories. The Monbusho reports a total of 300 cooperative projects linking universities and companies during 1987, 11 of them (all with Monbusho sponsorship) in superconductivity.[24] In some contrast to efforts in the United States, many of which seek to push universities into doing industrially relevant work (see the next chapter), rhetoric in Japan stresses cooperation in projects of interest to both sides.

Japanese leaders, like those in many countries, view ties among universities, industry, and government as weak. Statements on science and technology policy continually highlight the need for more effective working relationships. Industry tries to help by donating equipment to the universities, but professors worry aloud that industry will steal their best research workers. At the same time, senior professors typically help steer their graduates to particular companies, helping build long-lasting informal communications networks. Professional societies and study groups also bring people together, providing opportunities for working-level scientists and engineers from industry, government, and the universities to share information. In this respect, they replicate the function of high-level advisory committees involving senior professors, corporate executives, and ministry officials.

Recent changes in the law—for instance, making it easier for faculty members to consult—encourage interactions with industry, but many Japanese officials think further steps will be needed. Broad success in basic research would seem to demand such cooperation. Given the slow growth in the government R&D budget, the industry role is a critical one; superconductivity promises to be a prime test case.

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[24] "Sangaku Ittai e Hirogaru Koryu" [Expanding Exchange Between Industry and Universities], *Nihon Keizai Shimbun*, Jan. 20, 1988, p. 1.

As noted earlier in the chapter, many Japanese companies—Sumitomo Electric is a good example—have been expanding their basic research efforts, while also pursuing parallel programs of applied R&D in HTS. The new opportunities have pushed many firms toward more basic work—which they see as the necessary preliminary to commercialization—and sensitized them to the importance of university science. Even so, a major reorientation of Japanese R&D toward fundamental research will require institutional, cultural, and political shifts. The university system is widely viewed as hierarchical and stifling, offering inadequate incentives to bright young researchers. Change has begun, but it is not clear how far it will go or how deeply it will penetrate.

Rivalry or Cooperation? The Internationalization of Japanese Superconductivity R&D

International cooperation in HTS has been a central theme in pronouncements by government officials in Japan. MITI has opened its HTS programs to foreign companies. The STA states that it will promote international collaboration under the Multicore Project. In addition, the Key Technology Center—sponsored by MITI and the Ministry of Posts and Telecommunications—has provided financial support for foreign engineers and scientists who wish to work in Japan. Finally, the Ministries of Foreign Affairs and Education have announced a postdoctoral fellowship program that will bring recent graduates of overseas universities to Japan for research. Possibilities for U.S.-Japan cooperation in HTS also exist on an agency-to-agency basis. The U.S. National Bureau of Standards and ETL have been exchanging scientists for a number of years. Both have informally expressed interest in cooperation on HTS-related standards.

Why the stress on "internationalization"? There are two major reasons:

- The United States, along with other nations, has been pressing Japan to make a greater contribution to global welfare—one commensurate with the size of the Japanese economy and Japan's technological capabilities (box L). Among other things, this implies a greater commitment to science—the fruits of which should benefit all—and to the transfer of technologies to other parts of the world. Opening Japanese research institutions to greater foreign participation would be a first step. In many official policy statements—including those on HTS—Japan has pledged to take such actions.
- More than just altruism, internationalization would serve Japan's interests as well. Foreign scientists and engineers will help invigorate Japanese laboratories, encouraging new approaches to research, and breaking down some of the traditions which—particularly in the universities—seem roadblocks to creativity. Japanese leaders also realize that they may have to open their own doors to retain access to R&D from other countries. With science and technology holding the keys to continuing economic growth in the 21st century—a firm belief in Japan—internationalization can be viewed as a strategic and economic imperative.

The stress on international cooperation does not signify any slackening in Japan's efforts to develop indigenous technologies. The Japanese view it as a complement to these efforts—far more than a matter of image, it is an intrinsic element in Japan's strategy for competing in a world of intensifying global rivalries.

So far, as box L indicates, rhetoric has overshadowed results. MITI's pitch for international collaboration focuses on the International Superconductivity Technology Center, established in January 1988. ISTEC, which gets financial support from MITI, will be located near Tokyo on a site formerly owned by Tokyo Gas. More than 85 Japanese companies have signed on as founding members. Although the initial fee for full members is about $800,000, with annual charges of about $100,000, the costs are considered donations and earn the companies tax benefits. Associate members, who pay

Box L.—Prospects for U.S.-Japan Cooperation in Superconductivity R&D

President Reagan has called for bilateral cooperation in superconductivity with Japan. But despite apparent interest by the Japanese Government, concrete steps have yet to follow. There is a broader context: prolonged negotiations over the non-energy bilateral science and technology agreement between the two countries.

During these negotiations, the United States raised questions concerning Japan's willingness to carry more of the worldwide burden of basic science research.[1] U.S. officials pressed Japan to open its research laboratories to foreigners, expand fellowship programs, even pay for translations of technical articles into English—while also trying to tie the agreement to concerns over export controls and intellectual property protection. Part of the background: repeated assertions that Japan always comes out ahead in scientific cooperation—that these arrangements become one-way streets in which Japan gets more than it gives.

Unless the Japanese take genuine steps towards cooperation, they run the risk of the United States closing off its own research, to the extent this could be accomplished. However, the advocates of international cooperation in Japan could be discredited if they are seen as caving in to unreasonable demands—an outcome that could have deep repercussions for U.S.-Japan relations.

From a U.S. perspective, it would be ironic if science and technology exchanges were curtailed just as Japan begins to demonstrate parity in research. In principle, two-way flow offers substantial benefits to the United States, although it would take hundreds of American engineers and scientists, able to communicate in Japanese, to take advantage of the opportunities. For Japan, cooperative projects could, under the proper circumstances, bring fresh ideas into the research establishment, while also helping to maintain access to work overseas.

Nonetheless, the significance of the invitations for foreign participation in STA and MITI undertakings on HTS has yet to unfold. Although the STA has invited foreign organizations to participate in the New Superconductivity Materials Research Association, only the Delegation of the European Community has joined. STA officials are weighing amendments to existing legislation that would make it easier for foreign scientists to work in the agency's laboratories. So far, only a few foreign firms have joined MITI's ISTEC program. If foreigners find themselves unable to participate fully, resentment will grow. If, on the other hand, cooperative projects bring tangible benefits to the foreign concerns, they could have symbolic importance well beyond HTS.

For the United States, participation in Japanese research may be the only way to gain full access to results. Certainly, it will help Americans gain a more sophisticated appreciation of Japan's approach to R&D—with potential benefit for U.S. companies seeking to improve their own performance in product development and manufacturing. Learning from Japan's technology means direct and deep involvement in Japanese R&D.

To take advantage of new programs designed to bring foreign researchers into Japanese laboratories, Americans will need considerable support from their employers at home and their hosts in Japan. For cooperation to be meaningful, the Japanese will have to structure projects so that Americans can function as full participants. On the U.S. side, effective participation demands capable scientists and engineers who have learned to speak their host's language.

Cooperation in HTS could become a bellwether for the future. The United States needs greater access to Japanese science and technology in many fields. But there is a potential downside to cooperation with Japan in HTS. The best Japanese HTS R&D is likely to take place in corporate laboratories, rather than the government and university facilities in which visiting Americans would probably be working. Universities and national laboratories are generally stronger in the United States than in Japan. The Japanese, in short, could learn more from exchanges in HTS. U.S. policymakers will have to weigh this risk against the overall benefits of gaining access to the Japanese system, particularly laboratories like ETL and NRIM—benefits that include the informal contacts so important in R&D.

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[1] M. Sun, "Strains in U.S.-Japan Exchanges," *Science*, July 31, 1987, p. 476; M. Sun, "Japan's Inscrutable Research Budget," *Science*, Oct. 2, 1987, p. 22; E. Lachica, "U.S., Japanese Negotiators Deadlocked on Tapping Each Other's Technology," *Wall Street Journal*, Jan. 22, 1988; C. Rapoport, "Japan goes public with apparatus for basic experiments," *Financial Times*, Mar. 11, 1988, p. 10; "U.S., Japan Hammer Out Science Agreement," *Science*, Apr. 8, 1988, p. 140.

According to a survey by STA, 600-plus Japanese firms hosted 360 foreign scientists and engineers during 1986. The survey provides no information on nationalities, nor is there information on American or other foreign researchers temporarily working in Japanese universities or national laboratories. See Report Memorandum #141 (Revised), Tokyo Office of the U.S. National Science Foundation, Jan. 28, 1988.

much less, will not be able to participate in research or have immediate access to R&D results, but simply to ISTEC publications and symposia.

ISTEC plans not only to conduct research in its own facilities, but to support R&D in other institutions, review and evaluate research for its members, and carry out feasibility studies on applications of HTS. To benefit from full membership, foreign companies would need Japanese-speaking employees, skilled in relevant technologies, on site in the ISTEC laboratory. As of May 1988, no foreign companies had joined as full members, although several had signed on with associate status.

Are American companies missing a bet by not joining ISTEC? For smaller firms, the costs pose a major barrier. But a number of U.S. companies with R&D operations in Japan could certainly afford them. Some form of jointly sponsored membership—e.g., through an industry association, or a joint venture such as Microelectronics & Computer Technology Corp.—might also be possible. If ISTEC yields impacts comparable to past MITI-promoted R&D efforts —e.g., the very large-scale integrated circuit project of the late 1970s—then participation could pay off. Even if the results in terms of research outcomes prove meager, active participation helps keep tabs on the competition. This, after all, has been a primary motive for Japanese firms to join in such group efforts.

CONCLUDING REMARKS

The U.S. business culture differs from that in Japan. R&D strategies in American companies tend to be driven by hard-headed calculation of risks and rewards—which does not encourage aggressive commitments to HTS. Most U.S. firms hope that someone else will do the fundamental research. Many American executives feel uncomfortable with this short-term approach, sometimes defensive. But they see little choice, given the way U.S. financial markets operate.

Japanese executives work in a society and an economy with a different set of traditions and rules. They too must worry about profit levels, but these are not the most important influence on their behavior. Japanese managers think first about growth and market position. Furthermore, they are acutely aware that they can no longer depend on technologies from the United States and Europe. Managers in Japan are attempting, often with some fumbling, to increase their firms' research capabilities, seeing this as one road to continued expansion.

Given the differences in attitudes and in approach to R&D, HTS has stimulated contrasting responses. As a generalization, large Japanese companies have more people at work on HTS, doing a greater variety of things. Japanese managers see HTS as a technology of paramount importance for global competition in the 1990s and beyond. U.S. executives might agree, but they also see the risks and uncertainties more starkly. They believe commercial products are farther off—that HTS will remain in the laboratory for some years to come.

Thus, most U.S. R&D efforts could be described as selective and probing. In contrast, most of the Japanese efforts are relatively broad, with people already assigned to think about applications. In pursuing their strategies, Japanese companies are studying superconductivity *now* as a potential commercial technology. American companies are not. The Japanese companies could be wasting their time and money. At this point, no one knows. But if the pace of discovery in the future matches that of the past year, Japanese companies will be better positioned.

In the United States, some of the first HTS applications may well come from small, startup firms—financially weak, and likely to face difficulties in growing. The pattern is clear in biotechnology, where startups have had to link with larger companies to proceed with commercialization. Thus far, of course, the startups are outnumbered by big American companies like Du Pont and IBM. In Japan, the large, diversified, and financially strong companies have the field largely to themselves.

These Japanese firms are poised to move quickly into production and marketing, on a worldwide scale if they choose. In the past, this asymmetry in industrial structure has had powerful impacts—e.g., on competition in microelectronics, where American firms have fallen behind for reasons that include lack of financial muscle. It remains to be seen how the story will unfold in biotechnology—or in HTS.

In the United States, cooperation between Government, industry, and the universities tends to be *ad hoc*, motivated by particular circumstances. There is no indication that HTS will be an exception. Japanese companies compete intensely with one another, but are nonetheless quite capable of cooperating on projects judged to be in their interests, especially when MITI or government agencies seek to foster these projects.

Japanese Government funding for R&D in superconductivity will not match spending by the U.S. Government (although the exclusion of salaries from some of the Japanese budget figures makes comparisons difficult). Including both LTS and HTS, the U.S. Government will spend more than twice as much in fiscal 1988. More important, however, Japanese firms have many more people at work on HTS than American firms. Companies commercialize, and companies in Japan have stronger commitments to superconductivity.

Neither country has a coordinated national initiative. Both seek to promote cooperation among universities, industry, and national laboratories. While business and government in Japan do not always find it easy to cooperate, they do exchange views and work toward consensus. And, if Japan's policy cannot be described as a coordinated plan, policy directions have been debated much more thoroughly than here. By the beginning of 1988, policy objectives in Japan had been reasonably clearly defined. They show a clear recognition of specific needs and specific problems impeding commercialization, and the Japanese Government aims to help solve them.

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Chapter 4

U.S. Technology Policy: Issues for High-Temperature Superconductivity

SUMMARY

The preceding chapter discussed company strategies toward high-temperature superconductivity (HTS) in the United States and Japan, as well as the policies of the Japanese Government. The question now becomes: How can U.S. Government initiatives help American companies with commercialization? Both this chapter and the next deal with Federal policies and what they mean for HTS. Both also go beyond superconductivity, taking up broader issues that affect commercialization and competitiveness.

Many of these policy issues are matters of ongoing concern to Congress and the executive branch: the Federal R&D budget and its management; the health of university research; technology transfer from national laboratories to industry. Table 7 provides a guide to some 20 policy issues and options discussed in this chapter; tables 9, 13, and 14, which follow later, give more detail. As a glance at table 7 makes clear, many of the issues and options have relevance that goes far beyond HTS. By the same token, many of the policy questions important for HTS can only be understood in the broader context of U.S. technology policy.

Federal agencies will spend some $60 billion on R&D this year (ch. 2). Industry will spend about as much, with private firms also conducting more than half the Government-funded total under contract. All companies that use technology live to some extent off the publicly financed storehouse of technical knowledge. The path to commercialization begins with this technology base.

The overall size of U.S. R&D expenditures—more than twice as much as Japan, and far more than any of the Western European nations—presents something of a paradox. How is it possible, given spending on science and technology exceeding $125 billion, that the United States has a problem in technology? Why doesn't American industry have what it needs to compete? The question has two kinds of answers, both partially true. The first is that technology is not, in fact, the problem—that difficulties in commercialization and competitiveness lie elsewhere. The analysis in chapters 2 and 3 indicated that technology is part of the problem—though far from the whole problem. The second answer is that not enough of the R&D money goes toward commercially relevant technology development.

Any analysis of the Federal role in commercialization must begin with a look at how the Government spends its $60 billion.

- Nearly 70 percent goes for defense, up from 57 percent at the beginning of the Reagan Administration. The United States devotes a much larger share of total R&D outlays for military projects than most other countries. Defense gets less than 5 percent of the Government R&D budget in Japan.
- Much of the Federal money—this year, about $20 billion—goes to the 700-plus national laboratories. For the most part, these laboratories do not have a good track record in transferring technology to civilian industry. While recent initiatives by Congress and the Administration have sought to strengthen interactions between the laboratories and industry, the process of change is just beginning.
- Outside of defense, aerospace, and health, Federal agencies spend little on applied research and development. Given the short-term orientation of most of the R&D paid for by private industry, a wide gap often separates basic research and commercial technologies—a gap that neither Government nor industry has been filling.

Table 7.—Summary Guide to Policy Options

Issue area	Option	Relevance
I. Funding Levels and Priorities for Federal R&D (see Table 9 for details)		
A. Funding Levels for HTS		
• New money, agency priorities	1	HTS
B. Continuity of Funding		
• Multi-year benchmark plan	2	HTS, but potentially broader
• Two-year funding trial	3	HTS, could be broader
C. National Science Foundation Budget		
• Overall NSF budget increase	4	general
• Funding for university laboratory equipment	5	general
D. Weaknesses in the Industrial Technology Base		
• Review of U.S. technology base	6	all commercial technologies
• Basic research tax credit	7	general
E. Setting Priorities for Federal R&D		
• Strengthen the Office of Science and Technology Policy	8	general
II. Strengthening Interactions Among Universities, Industry, and Government (see Table 13 for details)		
A. University-Industry Interactions; Multidisciplinary Research		
• Funding for NSF centers	9	general
• Postdoctoral fellowships	10	general
B. Government-Industry Interactions: Technology Transfer and Joint R&D		
• Oversight on technology transfer from the national laboratories	11	general
• Pilot program for transfers of HTS technology resulting from DoD-sponsored R&D	12	HTS, but potentially broader
• Technology transfer demonstration projects	13	general
• Personnel exchanges	14	HTS could get special attention
• Cooperative R&D with industry	15	HTS, but potentially broader
• Sharing costs with private R&D consortia	16	HTS, but potentially broader
• Support for State Government initiatives	17	general, but HTS could get special attention
III. Technology Interchange with Japan (see Table 14 for details)		
• Seed grant for office in Japan to monitor developments in HTS	18	HTS
• Research participation and language training	19	general
• Japanese technical literature	20	HTS, but potentially broader

SOURCE: Office of Technology Assessment, 1988

At present, the Federal Government may be spending as much on HTS as the private sector. The agencies expect to spend $95 million on HTS in fiscal 1988. OTA's industry survey (ch. 3) found that 55 U.S. firms plan to spend about $97 million on superconductivity R&D (LTS as well as HTS) in 1988.

While $95 million sounds like a lot, nearly half will go for military projects. Department of Defense (DoD) objectives shape R&D goals even at the level of basic research. Nonetheless, **much of the fundamental understanding of HTS that results from DoD-sponsored research will support the overall technology base**

for HTS. Moreover, the Defense Advanced Research Projects Agency (DARPA) has emphasized processing in its HTS R&D; this work should yield commercial spinoffs.

In general, however, civilian and military technologies have been diverging, as DoD's needs grow ever more specialized. This pattern is already evident in HTS, where prospective applications include passive shielding for protection from nuclear radiation, or sensors for the Strategic Defense Initiative (app. B). Moreover, in a period of tight budgets, DoD decisionmakers—from project and program managers to laboratory directors and Under Secretaries—scrutinize the R&D budget to make sure that immediate military needs get the highest priority. Basic research suffers in such periods, along with other work that might be of use on the civilian side of the economy.

The Department of Energy (DOE) and its laboratories will get the lion's share of the nonmilitary funding—nearly 30 percent of the Federal total. Ten DOE laboratories may have more to spend on HTS in 1988 than NSF will distribute to the Nation's universities. DOE's basic research, like that of DoD, will help support the technology base. As for commercial technology, the laboratories are trying to develop new cooperative ties with U.S. industry. However, it could take years for effective working relationships to develop; in the absence of such relationships, DOE R&D may not make a major contribution to commercial technology development.

The National Science Foundation (NSF) share of the HTS R&D budget, going almost entirely for university research, declined from 25 percent of the Federal total in 1987 to 15 percent in 1988. The universities do get some funding from DOE and DoD (especially through the basic research programs of the Air Force and the Navy). But **the allocation of Federal R&D funds seems out of balance, given the great strength of American universities in basic research**.

Continuity of funding over the next 5 to 10 years will be just as important as the level and allocation in any one year (Options 1, 2, 3). The Federal budget for HTS is really nothing more than the cumulation of agency decisions and appropriations. Both Congress and the Administration could benefit from a better sense of the overall dimensions of the Federal effort, so that priorities could be weighed rather than simply emerging at the end of the yearly budget process. A benchmark, multi-year funding plan for HTS, which could be adjusted periodically (not at all a rigid blueprint), would help in making good decisions. Congress might also choose to experiment with multi-year authorizations and 2-year budgeting. These steps could help avoid too much duplication in agency R&D (some overlap can be desirable), as well as cuts in other needed R&D to provide money for HTS (little of the Federal total represents new money specifically appropriated for superconductivity).

Many fields of science and technology vital for competitiveness do not get adequate research support; technical knowledge that could help American firms compete is not available when they need it. Often, the underinvestment is most severe in fields that lack glamour and the promise of immediate payback (examples range from materials synthesis to corrosion and wear)—just those likely to suffer when more money must be found for an exciting new opportunity like HTS. Given the constraints on the Federal budget, any decision to begin filling some of these gaps by spending more on civilian R&D must begin with good information and a government-wide perspective, matters addressed in Options 6 and 8.

Commercializing HTS will require multidisciplinary R&D—physicists, chemists, materials scientists, and engineers. NSF can play a vital role in supporting multidisciplinary research in universities, where such work has seldom caught on (Options 9 and 10). While the Reagan Administration proposed doubling the Foundation's budget over a 5-year period, Congress gave NSF very little increase for fiscal 1988. Sustained growth in the NSF budget will be needed if the agency is to increase its support of traditionally underfunded areas, including engineering research—a critical priority for competitiveness.

If the Federal laboratories, in their turn, are to provide much help in commercialization, they will need to make sustained commitments to working with the private sector (Options 11 through 17). Congress, in several recent laws, has stressed the need for closer linkages between the laboratory system and industry. Agency responses have been mixed. With experience limited, it might be prudent for the DOE laboratories to adopt an experimental approach, beginning with pilot projects, rather than plunging into a full-fledged program of cooperative endeavors. Personnel exchange programs could also help shift the culture of the laboratories; scientists and engineers working in the laboratory system need to understand how industry functions and how the markeplace works if they are to help in commercialization.

Chapter 3 outlined Japan's proposals for international cooperation in HTS research. So far, American firms have not responded with much enthusiasm. Options 18, 19, and 20 suggest steps the Federal Government could take to help industry and professional groups test Japan's openness to foreign R&D participation, and to monitor Japanese technical developments. Given the importance of person-to-person contact in technology transfer, early steps should include language training for U.S. engineers and scientists, so they can work inside the Japanese research system.

Although the analysis that follows covers a broad range of issues related to HTS, it does not pretend to be a comprehensive discussion of U.S. technology policy. Nor do the 20 policy options address all the problems identified in earlier chapters—short-term decisionmaking in U.S. industry, for example. This chapter has a more modest aim: examining alternatives for managing the Federal R&D budget to more effectively support the Nation's commercial technology base without detracting from agency missions. Most of these are incremental policy adjustments; chapter 5 looks at more comprehensive alternatives.

GOVERNMENT SUPPORT FOR HTS R&D

Funding Levels

Funding for HTS R&D has grown dramatically since the end of 1986; table 8 gives the best available estimates.[1] It is hard to criticize the totals; indeed, the increases shown in table 8 seem generous in a time of budgetary pain. Although little of the money represents new budget authority, in 1988 the U.S. Government will probably spend more on HTS alone than Japan's Government will spend on HTS and LTS together. The 1988 total approaches the recommendation—$100 million—of a National Academy of Sciences (NAS) panel.[2]

But the totals do not tell the whole story. HTS could remain in the laboratory for many years. During much of this time, the Federal Government will remain a primary source of R&D funds. Effective support for commercialization will require stability in Federal funding, attention to priorities, and good management of agency budgets.

In their fiscal 1988 budgets for HTS, some agencies fared much better than others. DOE and NSF spent roughly equal amounts on HTS in 1987; the Energy Department will have more than twice as much this year, while NSF's in-

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[1] Low-temperature superconductivity (LTS) has shared in the expansion. For years, DOE and DoD have funded LTS projects such as energy storage and superconducting machinery (e.g., for ship propulsion—Appendix B). Federal spending for LTS increased from $40 million in fiscal 1987 to $64 million in 1988. Agency requests for LTS in the 1989 budget come to about $83 million. (Both the 1988 and 1989 figures include Strategic Defense Initiative (SDI) contract work on superconducting magnetic energy storage.)

[2] "Research Briefing on High-Temperature Superconductivity," Committee on Science, Engineering, and Public Policy, National Academy of Sciences, Washington, DC, 1987, p. 19. The panel, noting that corporate funding might add a comparable amount, termed this ". . . a good beginning in addressing the challenges and opportunities offered by the new materials."

Table 8.—Federal Funding for HTS R&D

	Fiscal year budget (millions of dollars)		
	1987 (actual)	1988 (estimated)	1989 (requested)
Department of Defense[a]	$ 19.0	$ 46.0	$ 63.0
Department of Energy	12.5	27.2	38.7
National Science Foundation	11.7	14.5	17.2
National Bureau of Standards	1.1	2.8[b]	9.3
National Aeronautics and Space Administration	0.5	4.2	6.7
Bureau of Mines	0.1	0.1	0.1
	$ 44.9	$ 94.8	$ 135.0

[a]Working figures, subject to change.
[b]Excludes $750,000 for related work.

SOURCE: Preliminary agency data and budget estimates provided to the Subcommittee on Superconductivity of the Committee on Materials, May 1988.

crease is only 25 percent. NSF officials have said they have received many more highly-rated proposals on HTS than they can support in fiscal 1988. Meanwhile, the DOE laboratories—which typically get nearly two-thirds of the Department's basic research funds—may have more money for HTS than NSF will provide the Nation's universities.

The NAS panel emphasized the need for new money for HTS to avoid cuts in other, perhaps comparably important, R&D. When the excitement over HTS reached a peak early in 1987, the fiscal year was well underway. Thus almost all the 1987 funding came through redirecting of dollars originally allocated to other research. In many cases, scientists and engineers with Federal contracts and grants took the lead in this process, seeking approval from agency contract monitors to move into HTS.

Faced with little growth in R&D budgets, most agencies have had little choice but to continue pulling money from other fields to pay for HTS. The National Bureau of Standards (NBS)—with a budget that grew 16 percent from 1987 to 1988— is probably alone in being able to fund its HTS work without sacrifices elsewhere.

In a period of tight budgets, when there may be no way to avoid sacrificing one kind of research to pay for another, good decisions on priorities within and across agencies become more important than ever. Doing a better job of formulating R&D budgets could help identify conflicts earlier, and perhaps ease their resolution. For HTS, stability over time will be as important as next year's R&D totals.[3]

At present, most of the funds for HTS come from the general R&D authorizations of the agencies. Rather than this piecemeal approach, Congress could take a broader look at the Federal effort in HTS, and provide overall guidance, through such mechanisms as a single piece of legislation that would provide multiyear authorizations of appropriations, defining the responsibilities in HTS for each agency. This approach is discussed in more detail in table 9 (**Option 1**). It carries dangers: for example, possible micromanagement by Congress. On the other hand, if implemented in too weak a form, the effort could end up as little more than a paper exercise, with little or no influence on the actual allocation of HTS R&D support across the agencies.

As a further step, Congress could direct the Administration to prepare a multi-year estimate of funding expectations for HTS R&D (see **Option 2** in table 9). Some of the proposals on HTS before the 100th Congress—e.g., H.R. 3217, as

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[3] In a well publicized episode, a recent NSF effort to reduce uncertainty in university research programs backfired. Managers in the Foundation's Materials Research Division, expecting a substantial funding increase in fiscal 1988, made too many long-term commitments during 1987. When the Federal budget was finally approved, and the money was not there, NSF cut back on ongoing multi-year grants (which are conditional on availability of funds) in order to support some new starts. See "Statement on Funding Levels for the Division of Materials Research," National Science Foundation, Mar. 3, 1988.

Table 9.—Issue Area I: Funding Levels and Priorities for Federal R&D

Issue	Options for Congress	Advantages	Disadvantages
A. Funding Levels for HTS On the surface, Federal funding for HTS R&D seems generous—$95 million for fiscal 1988. The difficulties lie beneath the surface: • Little of the total is new money. Few agencies got the increases in their R&D budgets they had planned on for fiscal 1988. They have taken money for HTS from other research. • Universities have had difficulty in lining up funds. Ten DOE laboratories may well get more for HTS during fiscal 1988 than NSF will have for all the Nation's universities. • The Administration is requesting a hefty increase for HTS—to $135 million in 1989—and is calling for a substantial rise in non-defense R&D. If Congress pares back the R&D budget to accommodate other needs, the new money issue, along with allocations of R&D funds among the agencies, could be central issues, not just for HTS, but for R&D generally.	**OPTION 1.** Provide a legislative framework defining the overall Federal commitment to HTS—for example, a single bill providing specific multi-year authorizations for HTS R&D by agency. The authorizations would signal the congressional appropriations and budgeting committees, as well as the agencies, concerning the relative shares of funds for HTS R&D to be given to each agency.	A single framework for funding decisions could help keep Congress aware of potential imbalances among the R&D agencies. Multi-year authorizations, along with the multi-year planning exercise discussed in Option 2, and the experiment in multi-year funding discussed in Option 3, could help make the point to universities, the laboratories, and to industry that Congress intends to sustain the Government's commitment to HTS over time.	Congressional guidance could turn into micromanagement of Federal R&D, or pork barreling.
B. Continuity of Funding HTS could easily require a decade or more of steady R&D support before a technology base adequate to support commercialization emerges, with a continuing need for Congress and the executive branch to assess funding levels, as well as allocations across agencies—e.g., support for processing R&D, and whether it is adequate to support commercialization.	**OPTION 2.** Direct the Administration to prepare a multi-year estimate of Federal funding expectations for HTS R&D. This might be a rolling 5- to 10-year plan, directed at commercial (rather than military) applications, and intended to be revised periodically (not a rigid, inflexible set of research targets). Private sector input could be built into the process.	As a mechanism for helping policymakers gain perspective on annual budget proposals, multi-year estimates should be useful both to Congress and the agencies. The effort could improve agency coordination, limit overlap in R&D funding, improve the quality of scientific and technical advice to Federal agencies, and reduce the likelihood that money for superconductivity will come at the expense of other needed R&D. If successful for HTS, the approach might become a model for other fields.	Without proper oversight from upper levels in the Administration, such an exercise could turn into an agency wish list, with little utility for making tough budget decisions. Moreover, any effort to develop a government-wide perspective would probably be seen by some as top-down planning—threatening agency autonomy and flexibility. Multi-year budget estimates, finally, would probably have limited utility unless the agencies supported the concept—which few do now.
Stop-and-go funding has been a common problem for U.S. science and technology policy—and a serious one—in part because of year-by-year budgeting for Federal R&D. A period without newsworthy research results could lead to a dry spell in HTS R&D budgets.	**OPTION 3.** Direct the Administration to experiment with a 2-year funding cycle for HTS—possibly beginning with a pilot program at NSF. (Section 201 of Public Law 100-119 encourages congressional committees to experiment with multi-year authorizations and 2-year appropriations.)	Uncertainty over funding for HTS during 1987 and early 1988, and particularly over the prospects for new money, made it hard for research groups in government, universities, and industry to plan, and delayed some projects. Such problems cannot be totally avoided in a fast-moving field like HTS. But a 2-year budget cycle would help keep R&D on a steady course.	In the absence of improvements in mechanisms for establishing R&D priorities, a 2-year budget cycle would do little to overcome the fundamental budgeting problems posed by competition for limited funds. To some extent, a 2-year cycle might reduce the flexibility of the system, with potentially serious consequences in periods of rapid technological advance.
C. National Science Foundation Budget Despite the Administration's announced objective of a doubling in the NSF budget between 1988 and 1992, the Foundation's fiscal 1988 appropriation grew by only 6 percent (compared with a request of 17 percent). NSF has had to postpone increases in funding for multidisciplinary R&D centers and for research in engineering, traditionally underfunded.	**OPTION 4.** Consider substantial increases in the NSF budget over the next few years. Budget increases along the lines of President Reagan's proposal for a doubling of the Foundation's budget over 5 years would permit NSF to double or triple its funding for engineering research—to the $400 million to $500 million level—without sacrifices elsewhere.	More money for engineering would be a major step, not only in commercializing HTS, but in supporting U.S. industrial competitiveness across the board. NSF will spend $171 million on engineering research in fiscal 1988, only 10 percent of the agency's research budget.	Given the size of the Federal budget deficit, a significant increase for one agency could well come at the expense of others. The increases in civilian R&D included in the President's fiscal 1989 budget request—$300 million for NSF, $400 million for DOE, $2.5 billion for NASA—cannot be accommodated within the framework agreement worked out between Congress and the Administration in late 1987 unless Congress adjusts other budget items downward.
Laboratory equipment in many American universities is inadequate for either research or teaching.	**OPTION 5.** Appropriate substantially more money to NSF—an added $100 million or more per year—for equipment grants to the Nation's universities for both research and teaching.	Gifts from the private sector can help, but the problem is far too big to be solved in this way alone. Government action would help improve the Nation's technological capabilities.	Unless accompanied by an overall increase in NSF's budget (see Option 4), more funds for equipment could cut into the Foundation's research budget.

Table 9.—Issue Area I: Funding Levels and Priorities for Federal R&D—Continued

Issue	Options for Congress	Advantages	Disadvantages
D. Weaknesses in the Industrial Technology Base			
Despite the size of the U.S. R&D budget, gaps open in the technology base where neither industry nor government provide support. Prior OTA assessments have pointed to some of the problems; many more certainly exist. The first step toward a solution is to characterize the weaknesses more fully.	**OPTION 6**. Request a detailed review of the U.S. technology base by the National Academies of Science and Engineering. Such a review might encompass: • funding levels for both basic and applied research across the broad range of scientific and technical disciplines important for industrial competitiveness, with particular attention to actual and potential bottlenecks and to technical fields (like manufacturing) that historically have been underfunded. • processes for setting research priorities and determining funding levels within and across Federal agencies.	Given the budget deficit, it is more important than ever that R&D decisions be based on sound analysis. Less glamorous, less visible fields tend to suffer most in such periods, with harmful impacts that show up only in later years, when the damage has been done.	Studying the problem without taking steps to solve it would accomplish little.
American companies conduct relatively little basic research. Under the Tax Reform Act of 1986 (Public Law 99-514) companies get a more favorable tax credit for basic research they fund in universities than for work performed internally. Both the general R&D tax credit and the basic tax credit for work sponsored at universities expire at the end of 1988.	**OPTION 7**. Permit a separate tax credit for basic research conducted within the firm. To have much impact, an in-house research credit would have to be as favorable as current rules applying to basic research paid for by industry but conducted at universities, and more favorable than tax credits for internal R&D under the 1986 tax act. A basic research credit could supplement the overall R&D tax credit if Congress decides to make it permanent for 1989 and beyond. If Congress lets the existing credit expire, a special provision might be crafted—perhaps on a trial basis—for basic research within industry.	A basic research credit for work within the firm would create stronger incentives for attacking technical problems that fail to excite much interest in universities.	Creating new tax credits runs counter to the spirit of tax reform, while enforceable guidelines for basic research could be difficult to define.
E. Setting Priorities for Federal R&D			
Competition for Federal R&D dollars seems bound to grow more intense, with conflicting demands between big science and small, defense and civilian R&D, and basic research and more applied work. Establishing priorities and sticking to them—e.g., weighing the pros and cons of expenditures such as required for a Superconducting Super Collider, or the National Aerospace Plane—requires a government-wide perspective. This is the job, in principle, of the Office of Science and Technology Policy (in the Executive Office of the President).	**OPTION 8**. Give the Office of Science and Technology Policy access to the staff resources and advisory processes needed, not only to monitor science and technology issues in the agencies, but to assume an effective decisionmaking role within the executive branch.	A strengthened OSTP would permit the Executive Office of the President to develop and articulate priorities for science and technology—backed up with analytical depth and detail that have not been possible, given the Office's current staff (about 30) and budget (about $1.9 million).	OSTP will have little influence unless the President wants it to. Lacking this, congressional action to strengthen the Office would make little difference.

SOURCE: Office of Technology Assessment, 1988.

introduced—would direct the executive branch to provide, on a one-time basis, a Federal program plan for superconductivity, including estimated funding levels by agency for a five-year period. H.R. 3217 would assign the overall responsibility to the Executive Office of the President, with roles for the Office of Science and Technology Policy and the National Critical Materials Council. It provides for consultation with the mission agencies, as well as universities and industry. The proposal would also create a more formal structure for coordination among agencies. (Box M discusses interagency coordination of HTS R&D.) Any effort to develop Government-wide estimates risks being seen as top-down planning—threatening agency autonomy, professionalism, and flexibility. Nonetheless, viewed as a mechanism for helping policymakers gain perspective on annual R&D budget proposals, multi-year estimates could be useful both to Congress and to the agencies.

Box M.—Coordinating the Federal Effort

Given the U.S. Government's highly decentralized approach to R&D, coordination has been a perennial issue. HTS promises to be no exception. With a wide-open scientific and technical agenda—and potential applications that range from a space-based strategic defense system to high-performance computing to electric power (Appendix B)—many agencies have good reasons for supporting R&D, and many organizations and research groups good reasons for seeking funds from Government. Decisions made in industry will determine when HTS reaches the marketplace. But government decisions—on R&D funding (for universities, for the national laboratories, for industry), procurement plans by the mission agencies (principally DoD), and a host of other matters—will affect the timing of commercialization.

Issues of coordination have technical dimensions and policy dimensions. Soon after discovery of the new materials, the Administration established a subgroup of its Committee on Materials (COMAT) to deal with superconductivity. COMAT itself is a committee of the Federal Coordinating Council on Science, Engineering and Technology, an interagency group chaired by the White House Office of Science and Technology Policy (OSTP). The new subgroup provides a forum for interchange among research administrators of major Federal R&D agencies. A number of the agencies, in turn, have established internal coordinating bodies, such as the working group within DoD which prepared the Department's 5-year R&D options paper (discussed later in the chapter). Within DOE, the Energy Materials Coordinating Committee has created a subcommittee on superconductivity. DOE, NBS, and NSF have joined with the Electric Power Research Institute and a number of utilities in a committee on electric power applications of HTS. And of course, in a relatively small and specialized field like superconductivity, program managers and contract monitors in the agencies normally know one another, and talk frequently. Informal exchanges of information, however, have little to do with the hard choices of setting priorities and making budgetary decisions.

Up to this point, coordination at high policymaking levels—of the sort that might help sustain the Federal commitment to HTS over the longer term—has been *ad hoc*. The Economic Policy Council (EPC) set up a working group to develop the President's 11-point initiative, but the EPC itself is an informal body, with no statutory basis. Chaired by the Secretary of Treasury, and lacking full representation from such major R&D agencies as DoD, the EPC has not played much of a role since the release of the initiative. OSTP itself, with a staff of about 30 people and a very broad range of responsibilities, finds itself continually pressed to keep up with issues that emerge on a day-to-day basis. The Administration's Wise Men's Advisory Group on superconductivity—announced in the President's initiative but not appointed until February 1988—will probably go out of business after delivering its report.

Given the circumstances—hardly unfamiliar ones in U.S. science and technology policy—several members of Congress have introduced proposed pieces of legislation calling for a national program on superconductivity. Advocates of some such program point to the many Federal agencies that are already funding HTS R&D, and cite the need for coordination to avoid the dangers, on the one hand, of duplication, and, on the other, of neglecting critical technical needs. They also argue the need for an overall plan, with benchmarks for Federal R&D spending over the longer term. On the other side are those worried about new layers of bureaucracy, who contend that the R&D agencies do a good job of coordinating their activities—because, after all, it is in their interests—and that HTS will be no different.

Perhaps the first question is this: Who could do the planning and coordinating? Is it realistic, for example, to expect the National Critical Materials Council (NCMC) to take this on (as proposed in several bills)? Created by an Act of Congress in 1984, thus far the Council has not been very visible. The Reagan Administration opposed its creation, and did not name the first members for more than a year. Only rarely has the NCMC enjoyed a professional staff of more than one. The Administration's 1989 budget would cut the Council's funding in half, from $350,000 to $175,000. Assigned in 1984 the task of developing a Federal program plan for advanced materials, the Council has yet to do so, although plans have been announced for a submission to Congress in the summer of 1988; as currently envisioned, the submission will be little more than a report reviewing Federal activities in materials (including superconductivity)—not a program plan.

Without major changes, the NCMC would plainly have difficulty in serving as a coordinating body for HTS. This could all change, of course, given an Administration committed to the idea of a National Critical Materials Council—willing to give it a staff, and listen to its advice.

How about private sector input to government planning processes? President Reagan's initiative created the Wise Men's Advisory Group (all five members are in fact men) to provide high-level policy guidance. Several private organizations hope to serve similar functions on a less formal basis (e.g., the Council on Superconductivity for American Competitiveness, headed by George Keyworth, former director of OSTP). A number of bills before Congress have proposed temporary commissions with representatives from government, industry, and universities. Others propose a body that would report periodically to Congress on policies for accelerating commercialization of HTS.

The decentralized U.S. approach to R&D implies ongoing coordination. Lacking this for HTS, there are real risks of a Federal effort adding up to less than the sum of its parts. Perhaps the primary point is that *coordination in the sense of information exchange has little to do with priorities*. The Federal Government has few mechanisms for sorting out R&D funding across agencies. Multi-year authorizations and a 2-year trial for HTS, as suggested in Options 2 and 3, could help Congress and the Administration establish and maintain priorities.

With Congress appropriating money annually for research programs that may go on for years, the ups and downs in R&D funding have also stimulated frequent proposals for multiyear authorizations and/or appropriations.[4] Although Congress has been reluctant to move in this direction, growing concern over the budget process as a whole has led to discussion of a two-year budget cycle. As a more modest step toward a longer-term perspective on R&D decisions, Congress could experiment with multiyear funding in a single agency—perhaps NSF (**Option 3**). The experiment might be undertaken by programs in, say, the engineering directorate or the materials research division—both of which support HTS.

Neglect by Government and industry of commercial R&D has slowed the passage of technology from laboratory to marketplace, harming U.S. productivity and competitiveness. Less glamorous fields, particularly in engineering, seldom attract funding commensurate with their potential economic significance. Chapter 2 stressed U.S. underinvestment in processing R&D; other examples include materials synthesis (box C, ch. 2).[5] For such reasons, and despite the huge U.S. investment in R&D, the technology base no longer seems adequate to support a competitive set of industries.

In government, lack of mechanisms for setting priorities, coupled with stop-and-go funding for some kinds of R&D, have contributed to the problems. Gaps and holes in the technology base emerge particularly in fields that Federal agencies—DoD, DOE, NASA (the Na-

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[4] For discussion of some of the possible mechanisms, see *U.S. Science and Engineering Base. A Synthesis of Concerns about Budget and Policy Development*, GAO/Rced-87-65 (Washington, DC: U.S. General Accounting Office, March 1987), pp. 22-34.

[5] On the lack of R&D in construction technologies, see *International Competition in Services* (Office of Technology Assessment, July 1987), pp. 138-144. Other examples include:

- Direct reduction of iron to steel.
- Railway technology. (Given the importances of rail transportation for the Nation's economy, support has been woefully inadequate compared to, say, aeronautical engineering.)
- Process control models for the fabrication of microelectronic devices.
- Theoretical foundations for software engineering. (Better understanding could lead to greater productivity in programming, helping break a major bottleneck in U.S. industry.)
- Fundamental understanding of combustion processes. (Environmental pollution from stationary powerplants, burning of solid wastes, and automotive engines costs the United States billions of dollars each year. Lack of a research base in combustion—in terms of thermodynamics, chemical kinetics, fluid mechanics, heat transfer—makes it difficult to develop inherently clean combustion processes.)
- Corrosion and wear. (These processes, so familiar and pervasive as to seem inevitable, have economic costs measured in billions of dollars annually; wear, in particular, has never attracted much scientific attention or research support.)

Also see *Directions in Engineering Research: An Assessment of Opportunities and Needs* (Washington, DC: National Academy Press, 1987).

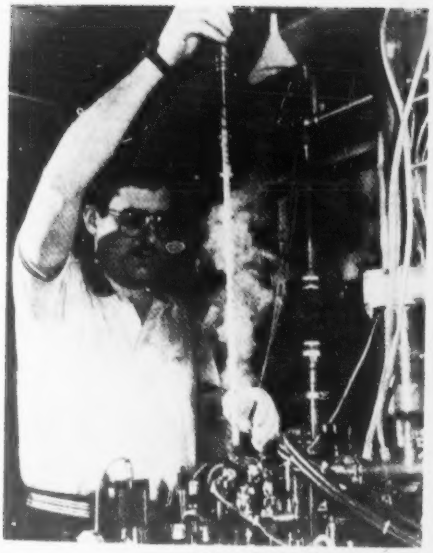

Photo credit Argonne National Laboratory

HTS superconducting wire, ready for testing.

tional Aeronautics and Space Administration)—view as too far from their missions, and that, in the view of corporate managers, will not yield financial returns in the short or medium term. Among the other causes: relatively low levels of support for engineering research, and Federal R&D programs that have often gone astray when not tightly linked to agency missions.

Congress could begin to enlarge the pool of commercially-relevant technology by appropriating additional funds to NSF, allowing the Foundation to expand its support for engineering research without taking money from other areas (**Option 4**). NSF's mission embraces the strengthening of the Nation's science and engineering base; yet its current spending on engineering research ($171 million) does not amount to three-tenths of a percent of the overall Federal R&D budget.

Congress might also provide additional money to NSF specifically for laboratory equipment. Equipment in the Nation's engineering schools averages 20-30 years old; a quarter of it cannot even be used.[6] An additional $100 million annually, to supplement NSF's current spending of $250 million a year—would help (**Option 5**).

NSF ranks no better than fifth in R&D spending among Federal agencies. Any search for a broad solution to the problems in commercial technology will have to look beyond NSF and the university research it sponsors. Given the pressures on the Federal budget, a realistic first step might be to identify the weaknesses in the existing technology base, and begin establishing priorities for allocating the limited funds available. Congress could ask the National Academies of Sciences and Engineering to begin this task (**Option 6**).

As a complementary measure, aimed at encouraging American firms to undertake more fundamental research, Congress might consider changes to the Research and Experimentation Tax Credit.[7] At present, industry finances only a fifth of all U.S. basic research. Federal agencies—which pay for two-thirds (universities fund the remainder)—do not set priorities based on commercial relevance. Giving companies greater incentives to conduct work inhouse would help focus basic research on industrial needs.

Congress could institute a special basic research tax credit for work conducted within industry (**Option 7**). Assuming that Congress extends the existing R&D tax credit, now set to expire at the end of 1988, or makes it permanent, basic research conducted internally could be given more favorable treatment than other qualifying R&D.

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[6] P. Doigan and M. Gilkeson, "Engineering Faculty Demographics: ASEE Faculty & Graduate Student Survey, Part II," *Engineering Education*, January 1987, p. 212. The National Research Council suggests that an increase of $30 million or more for engineering equipment alone would be appropriate—*Directions in Engineering Research: An Assessment of Opportunities and Needs*, op. cit., pp. 50-51. Also see "Scientific Equipment for Undergraduates: Is It Adequate?" staff paper, Science, Education, and Transportation Program, Office of Technology Assessment, Washington, DC, September 1986.

[7] Introduced in 1981, the credit was reduced from 25 percent of qualifying R&D expenditures to 20 percent in the 1986 Tax Reform Act. On its effectiveness, see *International Competition in Services*, op. cit., p. 364. Current law allows companies more favorable tax treatment for support of basic research at universities or other qualified R&D organizations than for work carried out at their own facilities.

Finally, Congress could ask the Academies for recommendations on an R&D strategy aimed specifically at strengthening the Nation's commercial technology base (as noted in Option 6). Such an exercise might help OSTP carry out its policy and planning functions—including legislative mandates that the office has had limited success in fulfilling. As discussed under **Option 8** in table 9, OSTP may need strengthening if it is to be an effective abritrator among agencies and interest groups seeking Federal R&D funds. In a period of intense competition for scarce dollars, a Government-wide perspective is needed more than ever in setting and enforcing priorities.

Defense-Related R&D

Funding Patterns

DoD has been supporting superconductivity R&D for more than three decades because of the potential applications in military systems. In this light, the dominance of DoD in Federal support for HTS (shown earlier in table 8) should be no surprise; much of the work is a natural follow-on to earlier sponsorship of LTS R&D.

The three services, together with DARPA and the Strategic Defense Initiative Organization (SDIO), maintain their own programs—with the DARPA and SDIO efforts the biggest by far (table 10). Three-fourths of DARPA funds, and a high proportion from SDIO, go to industry. DARPA states that as much as 60 percent of the processing R&D contracts currently in negotiation could go to firms that are not traditionally part of the defense industry. As for the services, about two-thirds of their HTS R&D funding is currently going to universities; if HTS follows the typical pattern for basic research in the services, this fraction may eventually decline somewhat (universities perform about half the 6.1 (basic) research paid for by the services, with government laboratories and industry sharing the remainder).

DARPA's widely publicized processing initiative accounts for nearly all that agency's 1988 total of $18 million. With no R&D facilities of its own, DARPA will support processing-related work in industry, universities, and laboratories overseen by other agencies. The primary objective: speeding development of fabrication techniques for HTS coatings, thin and thick films, wires and other conductors. DARPA officials view the effort as a natural extension of the agency's ongoing program in manufacturing technology for advanced ceramics. After receiving about 200 proposals during the summer of 1987—responses to a solicitation that assumed funding of up to $50 million for 1988—the agency announced in January that some 16 companies and 4 universities had been selected to enter into contract negotiations. When DoD placed a temporary freeze on some of its outside R&D (including DARPA's) in May 1988, nearly all of the contracts remained to be awarded. The freeze was in effect when this report went to press in June 1988.

SDIO's HTS R&D—second to DARPA's in funding—focuses on relatively near-term applications. The organization works closely with the services and other agencies, looking to "technology insertion working groups" for advice on where to direct its R&D dollars. Like other parts of DoD, SDIO contracts extensively with industry. In addition to HTS, the organization funds considerable work on LTS—for instance, a design competition on magnetic energy storage for powering large lasers, budgeted at $11 million currently and $13 million for fiscal 1989.

R&D sponsored by the services reflects their missions. Much of the Air Force effort goes toward possible applications in electronics, funded (principally through the Air Force Office of Scientific Research) in universities and the Air Force's own laboratories. The Office of Naval Research is likewise putting most of its current HTS money into basic research (6.1). While the Army also has a program underway, the level is low (as expected, given that the

Table 10.—Department of Defense Funding for HTS R&D[a]

	Fiscal year budget (millions of dollars)		
	1987 (actual)	1988 (estimated)	1989 (requested)
Army	$ 1.0	$ 2.0	$ 3.0
Navy	5.0	7.0	9.0
Air Force	4.0	7.0	8.0
Defense Advanced Research Projects Agency (DARPA)	4.0[b]	18.0	20.0
Strategic Defense Initiative Organization (SDIO)	5.0	12.0	23.0
	$19.0	$46.0	$63.0

[a] Working figures, subject to change. DoD also spends substantial sums on low temperature superconductivity.
[b] Includes $2 million from the Balanced Technology Initiative.

SOURCE: U.S. Department of Defense, April 1988.

Army traditionally funds relatively little R&D compared to the other two services). Each of the services has formed internal working groups to coordinate its effort.

Implications for HTS

With DoD paying for nearly half the government's HTS R&D, the obvious question follows: What does this mean for commercial development, and for the civilian side of the economy? In the past, Federal dollars for both R&D and procurement provided much of the impetus for vibrant commercial industries—aircraft, computers, microelectronics.

At the same time, as summarized in box M, DoD's very success in driving technology forward has led to a split between military and civilian applications, with defense systems growing steadily more specialized. Some would claim that military spending has undermined U.S. industry—distorting the technological enterprise by diverting the best and brightest engineers and scientists from civilian industries, skewing university research (and, through the research interests of faculty, university curricula), and turning companies aside from the cost-driven discipline of the marketplace. In this view, rather than providing fertile ground for spinoffs, DoD support for HTS might divert resources from commercialization.

Indeed, there seems little reason to expect that spinoffs from DoD funding for HTS will have impacts as significant as those that spurred earlier high-technology industries. Since the 1950s and 1960s, technology transfer from the military to the civilian side of the economy has slowed, for reasons that include the expanding curtain of secrecy surrounding DoD and its contractors. With military systems growing steadily more esoteric, it would be unwise to rely on DoD support for HTS as a *substitute* for civilian R&D. This does not mean that DoD R&D cannot be a valuable *complement*.

Two broad questions will determine the effects of DoD spending on the commercial prospects for HTS: 1) What are DoD's objectives with respect to HTS, and how do they compare with commercial needs? and 2) How much money will go to generic R&D, and thus offer potential for commercial spillover regardless of ultimate system requirements?

In mid-1987, a DoD working group examined the R&D that would be needed to exploit HTS in military systems. The working group, in an options paper described as a "map of the territory" rather than a "predetermined itinerary," concluded that an aggressive program to bring HTS to the point of military-specific applications would cost about $500 million over a 5-year period.[8] The working group's options paper, which assumes that technology, not money, would be the limiting factor, discusses R&D in several broad categories:

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[8] "Superconductivity Research and Development Options: A Study of Possible Directions for Exploitation of Superconductivity in Military Applications," U.S. Department of Defense, July 1987. Summary figures for the 5-year program plan, totalling $506 million, appear on pp. 122 and 123. In the first 3 years (fiscal 1988 to 1990), the working group called for $293 million—twice the $150 million DoD expenditure mentioned in the President's July 1987 superconductivity initiative, and far more than defense agencies are likely to spend over this period, judging from preliminary budget figures.

- materials characterization, including efforts to find HTS compositions with higher transition temperatures;
- processing R&D;
- small and large scale applications and demonstrations.

While there are no signs that the 5-year spending plan will go forward as outlined in the working group's report, the budget estimates provide a baseline for considering DoD's view of prospects and priorities in HTS. Sixty percent of the 5-year total would go for applications—$306 million. Processing—which holds more potential for commercially relevant R&D results—would get $129 million, or 25 percent; the options paper allocates $71 million for materials characterization, equally generic. The breakdown by budget category paints a similar picture: basic research (6.1) accounts for 29 percent of the total, compared to 38 percent for exploratory development (6.2), and 33 percent for advanced development (6.3A). Viewed either way, basic research and generic R&D would get a substantial share of the resources, as befits a new technology.

Most but not all of the applications work would be of interest primarily to the military. Examples include infrared sensors, detectors for submarines, and electromagnetic coil/rail guns. Some applications projects might generate commercial spinoffs: electronic devices for digital systems; motors, generators, and other electrical power equipment. (As discussed in app. B, these applications could, in principle, be implemented with LTS technology; indeed, even were HTS reduced to practice, LTS might provide superior performance.)

Still, superconducting motors and generators for military applications, to take one example, will differ fundamentally from those for civilian applications. DoD's interest stems largely from the advantages that superconducting motor-generator sets could have for ship propulsion and on board aircraft. Such propulsion systems would offer new freedom in packaging the major systems within a ship's hull; for submarines, in particular, there would be more room for weapons. Compact design becomes a primary design criteria. For civilian power generation, in contrast, greater efficiency is the objective, with size (and weight) of little import. From a design standpoint, superconducting generators for the military and for electric utilities would have relatively little in common. Only in the most general sense would knowhow from one transfer to the other.

Processing technology will be particularly important for HTS. Wire manufacture and fabrication received little emphasis in LTS R&D until becoming a bottleneck to applications. Years were then spent learning to produce niobium-titanium wires and windings with the needed properties. A similar experience in HTS could put U.S. firms behind, given that processing is an area in which Japanese firms will undoubtedly excel. Here, DARPA's processing program should help. Many of the processing and fabrication methods ultimately developed will be similar regardless of end-application, and DoD officials have frequently stated that results will remain unclassified to the extent possible. (In part for such reasons, H.R. 3024, the proposed National Superconductor Manufacturing and Processing Technology Initiative, would give DARPA a lead role in the Federal Government for processing-related work. The 100th Congress had taken no action on this bill, which assigns subsidiary roles to DOE, NSF, and NBS, as OTA's report went to press.)

DoD work aimed at high-performance computers, where applications will depend in part on thin-film fabrication capabilities—e.g., for Josephson junctions—could likewise have positive impacts on the civilian econony. Not only DARPA, but the National Security Agency has traditionally supported work aimed at high-performance computing (box N).

If DoD were to follow a spending plan something like that outlined by the working group—i.e., roughly half a billion dollars over five or six years—civilian industry would surely benefit from some of the technology developed. Despite the stress on applications—noteworthy, given the relative pessimism of U.S. industry

Box N.—DoD and Postwar High-Technology Development

R&D and Procurement

The decades between the end of World War II and the Vietnam War represented a kind of golden age for U.S. military R&D. Defense planners needed jet engines for high-performance fighters and bombers, computers for plotting missile trajectories, semiconductors for the guidance systems in those missiles. When these technologies were young, with almost no infrastructure of trained people or knowledge, the Federal Government picked up most of the tab for creating an industrial base. Defense and space agencies funded basic research, along with generic technology development. Within DoD, much of the responsibility fell to the Advanced Research Projects Agency, established in 1958 (the prefix changing the name to DARPA came in 1972). Box O, later in the chapter, describes some of ARPA's activities in support of university research during the 1960s.

(D)ARPA was not alone in supporting generic technologies. During the 1950s, the Air Force spent more than $60 million developing numerical control (NC) systems for machine tools—badly needed for carving out aircraft structural members. Work carried out at MIT's Servomechanism Laboratory and elsewhere led to both hardware and software technologies for NC that have since spread worldwide. To take another example, the SAGE air defense system, also developed for the Air Force during the 1950s, required the coordination of multiple computers in real time. Teams from MIT, IBM, Burroughs, and Bell Telephone Laboratories laid many of the foundations for timesharing, digital communications, and computer graphics.[1] Although NASA (and its predecessor, the National Advisory Committee on Aeronautics, NACA) focused primarily on system-oriented development, during the Apollo years the space agency took care to spend money with the Nation's universities.

The Pentagon not only paid for mission-oriented R&D directly, American companies pursued related work on their own in hopes of future contracts. As the customer, DoD guided and stimulated technological advance. Agencies knew what they wanted. They could evaluate alternatives, provide feedback to R&D groups and manufacturers.[2] For industry, the follow-on procurements meant profits and stability. Even in the middle 1960s, IBM got nearly half its revenues from sales to the U.S. Government—far more than any of the firm's rivals, and a major factor in IBM's emergence as by far the biggest computer manufacturer in the world.[3] In the semiconductor industry, learning economies resulting from rapidly expanding production volumes drove down the average price of an integrated circuit from $50 in 1962 to $1-$2 by the end of the decade. Missile systems accounted for most of the demand.

Indirect government support—e.g., purchases of computers by aerospace firms—also contributed to industrial expansion and technological advance. Companies like Boeing and Lockheed pioneered many of the emerging applications of computing. They bought machines, and learned to use them—generally writing their own software. The know-how spread quickly through the engineering profession and to U.S. industry as a whole. In essence, government and industry shared the risks of pushing the technology forward.

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[1] Much of the information on computers in this box comes from "Government's Role in Computers and Superconductors,", prepared for OTA by K. Flamm under contract No.H3-6470, February 1986. Also see K. Flamm, *Targeting the Computer* (Washington, DC: Brookings, 1987) and *Creating the Computer* (Washington, DC: Brookings, 1988).

Although the SAGE project proved an expensive white elephant from a strictly military standpoint, DoD came to regard it as a great success because of the technologies it spawned.

On NASA and the universities, below, see "Case Studies of 'Flagship' Technology," prepared for OTA by W.H. Lambright and M. Fellows, Syracuse Research Corp., under contract No. H3-5565, Dec. 31, 1987, p. II-6.

[2] While goal-oriented military R&D often led to successes like the Minuteman missile guidance and control system, in other cases DoD has followed system objectives down technological blind alleys. From the invention of the laser in 1960, for example, the military pursued high-power solid-state lasers for applications such as ballistic missile defense. When the gas dynamic laser appeared on the scene in 1966, the services were already heavily committed to the solid-state alternative. Although gas-dynamic lasers held much more promise technically, it was several years before the services changed direction. See R.W. Seidel, "From glow to flow: A history of military laser research and development," *Historical Studies in the Physical and Biological Sciences*, vol. 18, 1987, p. 111.

[3] *International Competitiveness in Electronics* (Office of Technology Assessment, November 1983), p. 147.

On integrated circuits, below, see N.J. Asher and L.D. Strom, "The Role of the Department of Defense in the Development of Integrated Circuits," IDA Paper P-1271, Institute for Defense Analyses, May 1977.

Finally, DoD also funded a considerable amount of visionary research—one of (D)ARPA's jobs. Here, the military mission did not always dictate R&D objectives, or even provide much guidance: (D)ARPA supported work in artificial intelligence and the behaviorial sciences in the absence of near-term military applications.

Military and Civilian Technologies: Diverging Objectives

During the Vietnam years, defense R&D growth slowed; DoD has never built its support for generic technology development back to pre-Mansfield Amendment levels. (The Mansfield Amendment, part of the military authorization bill for fiscal years 1970 and 1971, sought to tie DoD R&D more closely to defense needs.) Meanwhile, military high technology moved steadily away from civilian high technology. In the face of pressures from the Pentagon, DARPA too has turned toward projects for which it can more easily demonstrate military relevance, and steered a greater fraction of its funding to traditional defense contractors.[4]

As computers, for example, proliferated on the civilian side of the economy, prices dropped and the government role as primary customer declined. Computer firms took more of the R&D burden on themselves, adapting their products to the needs of banks, insurance companies, and manufacturing firms. Even so, defense agencies have continued to support both basic research and high-risk, high-cost development projects—work that could have major impacts in the future; as noted in chapter 3 (box J), the National Security Agency provided partial support for IBM's research on superconducting computer components. Military demand also continues to provide substantial support for supercomputer manufacturers.[5]

In semiconductors, the story is similar. Military procurements accounted for about half of all U.S. production in 1960. By the middle 1970s, the military had become no more than a minor customer for all except the most highly specialized chips; today, military sales run at less than 10 percent of the U.S. market. In 1979, the Pentagon found itself forced to create the VHSIC (Very High-Speed Integrated Circuit) program, an effort to take advantage of advances on the commercial side of the industry, where applications had long since outrun those in military systems.

The Pentagon likewise provided much of the early R&D support for lasers—in the early 1960s, twice the industry's own spending—and today continues to pay for most of the work on high-power lasers.[6] Military R&D, including fundamental research, has been conducted primarily in DoD's own laboratories, or those of its contractors, not at universities. As customers, the services have sought laser rangefinders for tanks—the first significant application on the defense side—and beam weapons. Civilian applications, meanwhile, began with eye surgery.

Today, the growing divergence between military and commercial technologies is visible in at least three ways:

- *System Design Requirements.*—In the 19th century, military needs contributed to the technology of interchangeable parts and the American system of manufactures, in the 20th, to the 707—but also to the Space Shuttle, and potentially to the recently proposed National Aerospace Plane (NASP).[7] Mission-specific operating requirements, and growing system complexity, mean less overlap between military and civilian designs. DoD's performance targets for the NASP go well beyond the point of diminishing returns for commercial carriers, who have shown little interest. Specifications and testing procedures for military chips (temperature cycling, radiation hardness) provide another example of the ways in which defense requirements may run counter to commercial needs; for years, the industry has argued that DoD requirements are unrealistic, but to little avail.

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[4] DARPA has weathered a number of these cycles—tolerance for visionary research, followed by a turn back toward applications, engineering, and hardware. See *Targeting the Computer*, op. cit., p. 109; also "The Advanced Research Projects Agency, 1958-1974," Richard J. Barber Associates, Washington, DC, December 1975.

[5] G. Kozmetsky, "Supercomputers and National Policy: Maintaining U.S. Preeminence in an Emerging Industry," *Supercomputers: A Key to U.S. Scientific, Technological, and Industrial Preeminence*, J.R. Kirkland and J.H. Poore, (eds.) (New York: Praeger, 1987), p. 10.

[6] "The Maturation of Laser Technology: Social and Technical Factors," prepared for OTA by J.L. Bromberg, The Laser History Project, under contract No. H3-5210, January 1988; R.W. Seidel, "From glow to flow: A history of military laser research and development," op. cit.

[7] *Advanced Materials by Design: New Structural Materials Technologies* (Washington, DC: Office of Technology Assessment, June 1988). Those on the commercial side of the aircraft industry envision an airplane that could fly halfway around the globe in 2 hours, reaching speeds of Mach 5 (i.e., 5 times the speed of sound). DoD sees the NASP as a possible launch vehicle for SDI, among other things, and the military version would have to reach Mach 25.

- *Manufacturing Processes.*—Much of what DoD buys, it buys in small quantities—a few ships, a few hundred planes, a few thousand missiles. Production costs may not be irrelevant, but mass production manufacturing technologies are seldom called for. Moreover, much of DoD's support for advanced manufacturing methods has gone toward specialized techniques such as diffusion bonding of titanium, or filament winding for graphite-epoxy rocket motor casings. Subsonic aircraft do not need titanium structural members, and much of the graphite-epoxy consumed by commercial industry goes into tennis rackets, fishing rods, and golf clubs (where the quality control requirements, for example, need not be as stringent).

  Despite a good deal of military R&D on automated manufacturing technologies—much of it following from earlier support of NC machining—the more recent benefits on the civilian side have been small. The Air Force program on Integrated Computer-Aided Manufacturing—launched in 1979—has yet to have much impact. All three services operate ManTech (Manufacturing Technology) programs. However, these do not seem to have had much effect, even in defense production.[8] DoD's emphasis on system designs that stretch the outside limits of performance encourages labor-intensive manufacturing methods, while cost-plus contracts do little to encourage efficiency.
- *Markets.*—Before the Cold War era, military planners viewed industrial mobilization much as they did manpower mobilization. Industry would convert from producing shoes and cars to boots and tanks. Nuclear weapons, fire-and-forget missiles, and airplanes packed with computers have changed all that. Today, it takes years—sometimes decades—to design and develop weapons systems. The result? A permanent defense industry, one that goes its own way, insulated from commercial markets.

  Even in the many large firms with a foot in both markets—Boeing, IBM, General Motors (especially since its purchase of Hughes)—the two sides of the organization normally function as separate entities; scientists and engineers, who once moved freely between military and commercial projects—taking new technology with them—now tend to stay on one side of the house or the other. In the 1980s, few defense contractors, shielded from the pressures of the marketplace, have the organizational skills or motivation to succeed at commercialization for civilian markets.

The Cold War also brought restrictions on exports of defense-related technologies and the diffusion of technical knowledge. Conflict between secrecy and technology transfer is not new: in 1947, when scientists at Bell Laboratories invented the transistor, they feared their discovery might be classified. Today, the conflict has become more severe. DoD's black programs consumed an estimated $22 billion in 1987, compared to $5.5 billion in 1981.[9] Defense agencies can and sometimes do forbid publication of results based on R&D they fund, even if unclassified. The effects of restrictions on the flow of scientific and technical information have been widely debated in the United States. The impacts on commercialization, while getting less attention than those on the research enterprise itself, could be just as serious.

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[8] *Manufacturing Technology: Cornerstone of a Renewed Defense Industrial Base* (Washington, DC: National Academy Press, 1987), p. 16.
[9] J. Stowsky, "Competing With the Pentagon," *World Policy Journal*, vol. 3, 1986, p. 701.

—the options paper calls for a lot of money in total, and a hefty infusion of funds for the more generic work.

But DoD will almost certainly *not* have this much money for HTS, as table 10 indicates. The fiscal 1988 total—$46 million—is well under the $68 million called for in the options paper, and the gap will grow: DoD has requested $63 million for HTS in fiscal 1989, much less than the working group's recommendation. With funds tight, defense agencies normally preserve their applications programs as best they can; they will have to continue with materials characterization and processing to support downstream development in HTS developments, but the temptation will be to go no further into basic work than absolutely necessary.[9]

The final point is this. R&D management in any mission agency entails a continuous series of large and small decisions. These deal with such matters as funding levels and priorities, research targets, intramural versus extramural projects. Contract and program managers constantly weigh alternatives for expenditures ranging from a few thousand dollars to many millions. Broad objectives are set at upper levels; people lower down make their choices guided by these objectives (though often with considerable autonomy). But *at all levels, DoD decisionmakers—from program managers to laboratory directors and the Under Secretary for Acquisition (who has overall responsiblity for DoD R&D)—have their eyes on military needs, not those of the civilian economy*. This is their job. Directives from outside the Pentagon may influence these day-to-day decisions, but not by much.

HTS R&D in the Energy Department Laboratories

DOE laboratories have actively sought major roles in HTS, typically for reasons including diversification beyond their primary missions. As table 8 indicated, DOE's budget for HTS R&D exceeds that of NASA, NSF, and NBS combined; table 11 gives the allocation within the Department. If usual patterns prevail, two-thirds or more of DOE's basic research dollars will be shared among DOE's nine multiprogram laboratories (the "National laboratories") and a number of more specialized research facilities.

The Energy Department and its predecessors have been the patron of big science in the Federal Government since the days of the Manhattan Project. While the Federal Government owns the DOE laboratories, most are operated under contract—some by universities, some by private corporations.[10] The laboratories have a collective budget well into the billions, and employ about 15,000 scientists and engineers. Several have strong foundations in superconductivity, stemming from years of work on LTS magnets for high-energy physics and fusion research, along with projects such as Brookhaven's 10-year effort on superconducting power transmission. By one estimate, DOE has spent $100 million on LTS R&D over the last two decades, in addition to $200 million for purchases of materials and equipment. Given this history, and the Department's responsibilities for energy R&D, it is no surprise that the laboratories have garnered the majority of non-DoD Federal dollars for HTS.

A number of the laboratories have excellent equipment for synthesizing and characterizing the new HTS materials. They have physicists, chemists, and engineers with the skills and experience to contribute to the science and technology base for HTS. But while many of these laboratories produce excellent science (as well as mission-oriented weapons development), they have little experience in helping industry

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[9] The technical objectives of DoD 6.1 basic research are commonly shaped to considerable extent by military needs. DoD's own options paper notes:

> While DOD will surely benefit significantly from efforts of other organizations (DOE, NSF, DoC, NASA) in areas of materials characterization, theory, and search for high-transition-temperature materials, it is essential that DSRD [Defense Superconductivity Research and Development] itself include substantive activity in these areas. Much of the remainder of DSRD activity is so highly applications driven that DSRD characterization, theory, and search activities are essential as a means to provide focus in directions of greatest perceived impact on DoD applications. Weight considerations are paramount in many DoD applications (as in those of NASA), and DoD has other stressing requirements related to mechanical and thermal shock, as well as to radiation hardness, all of which dictate that DoD-specific characterization investigations be pursued.

"Superconductivity Research and Development Options: A Study of Possible Directions for Exploitation of Superconductivity in Military Applications," op. cit., pp. 19-20.

[10] Eight multiprogram national laboratories have gotten most of the DOE funds for HTS. These laboratories, and contractors as of 1988, are:

Laboratory	*Contractor*
Argonne	University of Chicago
Brookhaven	Associated Universities, Inc.
Lawrence Berkeley	University of California
Oak Ridge	Martin Marietta Energy Systems, Inc.
Pacific Northwest	Battelle Memorial Institute
Lawrence Livermore	University of California
Los Alamos	University of California
Sandia	Sandia Corp. (a subsidiary of AT&T Technologies)

Livermore, Los Alamos, and Sandia are weapons laboratories. Single-program DOE laboratories active in HTS include Ames Laboratory (operated by Iowa State University) and the Solar Energy Research Institute (operated by the Midwest Research Institute).

Table 11.—Energy Department Funding for HTS R&D[a]

	Fiscal year budget (millions of dollars)		
	1987 (actual)	1988 (estimated)	1989 (requested)
Office of Energy Research			
Basic Energy Sciences	$10.2	$15.1	$16.7
High Energy & Nuclear Physics	0.2	0.2	0.3
Defense Programs	1.6	6.7	6.7
Office of Conservation & Renewable Energy			
Energy Storage & Distribution	0.2	4.4	12.9
Energy Utilization Research	0.1	0.4	2.0
Office of Fossil Energy Advanced Research & Technology Development	0.2	0.3	0.2
	$12.5	$27.2	$38.7

[a] Excluding the Department's Small Business Innovation Research Program. DOE currently spends more on LTS R&D than on HTS—$28.5 million on LTS in fiscal 1987, $39.5 million in 1988. In fiscal 1989, the Department is seeking $52 million for LTS R&D (a figure that excludes $34 million for procurement of materials and components).

NOTE: Totals may not add because of rounding.

SOURCE: U.S. Department of Energy, 1988.

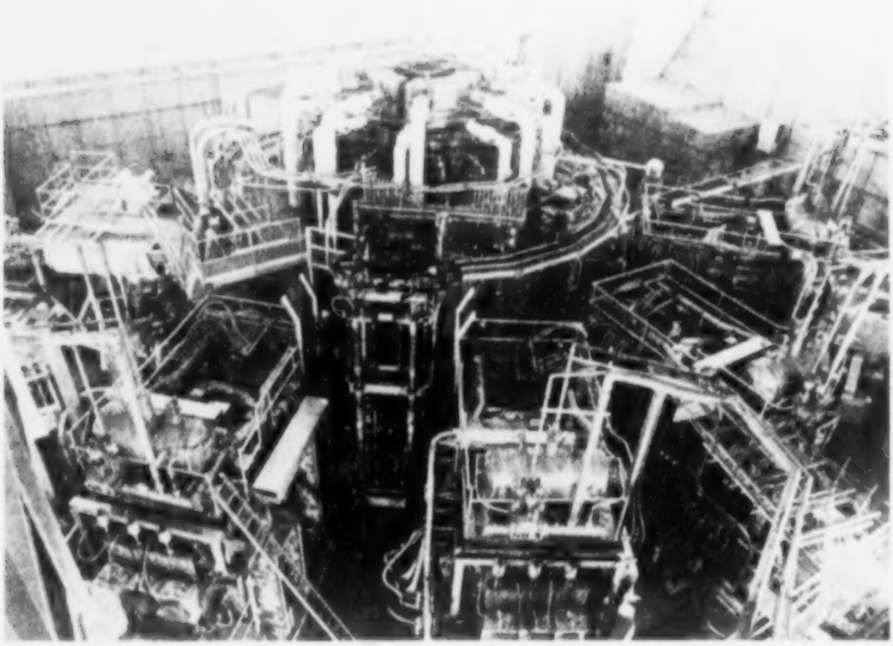

Photo credit: Princeton University Plasma Physics Laboratory

Tokamak Fusion Test Reactor, employing superconducting magnets.

commercialize new technologies. ***DOE plans to require the laboratories to involve industry and the universities in their HTS work to a greater extent than usual; for DOE's R&D to have impacts on commercialization commensurate with the Department's budget allocations, these efforts will have to succeed.***

In 1988, more than half of DOE's HTS budget—$15 million of $27 million total—will be channeled through the Basic Energy Sciences program (BES, table 11). While some BES funds go to universities and to industry, most of the program's HTS work during 1987 was undertaken within the laboratory system—a pattern that will probably continue.[11] BES has established two joint programs in HTS, each involving three laboratories. Under an arrangement worked out in 1987, Argonne, Ames, and Brookhaven will concentrate on processing R&D for bulk materials, while Oak Ridge, Los Alamos, and Lawrence Berkeley will work primarily on materials synthesis, thin films, and electronic devices. The Administration's 1989 budget request would give BES a 10 percent increase for HTS.

Another DOE office—conservation and renewable energy—will spend nearly $5 million in fiscal 1988 for R&D related to possible electric power applications. Initial activities included a number of feasibility studies, including a jointly funded effort with the Electric Power Research Institute examining possible end uses. If the President's 1989 budget is adopted, conservation and renewable energy could find its HTS budget tripling. Most of this would go to the office's energy storage and distribution group. In April 1988, DOE announced that it would provide relatively small sums to 10 DOE laboratories (eight of the multiprogram facilities, Ames Laboratory and the Solar Energy Research Institute) for work related to electric energy storage and distribution. Future funding under this program will depend in part on the ability of the laboratories to involve industry and universities.

As table 11 shows, the only other DOE program with significant funding for HTS engages in defense R&D. Most of this work—budgeted at $6.7 million for 1988, with next year's request at about the same level—takes place at the three weapons facilities.

The sections of this chapter dealing with technology transfer consider DOE's prospective contributions to commercialization of HTS—for instance, the likelihood of productive collaborative efforts between the Department's laboratories and private industry. If cooperative arrangements and rapid technology transfers to industry are to flourish, the laboratories will have to change in style and culture. Table 13, later in the chapter, includes a number of specific policy options for accelerating this shift.

Other Mission Agencies: NBS and NASA

For more than three decades, the National Bureau of Standards, part of the Commerce Department, has been engaged in research on LTS materials. President Reagan's superconductivity initiative gave NBS the responsibility for establishing a superconductivity center focusing on electronic applications. While NBS's technical achievements have been impressive—e.g., a precision voltage standard incorporating 19,000 Josephson junctions—the Bureau is small compared to many other Federal laboratories, and superconductivity a minor part of its work. The NBS appropriation for 1988 included $2.8 million for HTS projects (table 8) on measurement methods, standard reference materials, and devices for measuring weak magnetic fields. The Administration seeks a major increase for NBS—to $9.3 million—for fiscal 1989.

The National Aeronautics and Space Administration's HTS R&D will aim at eventual applications such as remote sensing, power and propulsion, and space communications.[12] In space, simple passive cooling systems could keep the new materials below their transition temperatures. As a result, HTS holds considerable interest for NASA. At the same time, space missions demand very high reliability, thus painstaking development and testing; deployment on an actual mission is probably many years in the future. In some contrast to the other major R&D agencies, NASA has not rushed into HTS; the agency's R&D is still in the planning stages.

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[11] The Division of Materials Sciences, which controls most of the money for HTS within BES, spent 63 percent of research funds totaling $155 million within DOE's own laboratories during fiscal 1987. About 35 percent went to universities (including support for graduate student research at national laboratories), and 1.8 percent to industry. See *Materials Sciences Programs: Fiscal Year 1987*, DOE/ER-0348 (Springfield, VA: National Technical Information Service, September 1987), p. F-3. These figures do not include $15.5 million in equipment funds.

NASA reprogrammed some $4.2 million for HTS during fiscal 1988—mostly for feasibility studies (table 8), and is seeking twice as much for 1989. The preliminary program plan cited above calls for spending $48 million on HTS over the period 1988-94. Even at this level, however, it seems unlikely that NASA R&D would have much impact on commercialization of HTS: mission requirements are apt to be too specialized.

NSF AND THE UNIVERSITY ROLE

The National Science Foundation is a mission agency too, but its responsibilities differ greatly from those of DoD, NASA, or DOE. The NSF mission: to support research because this is in the public interest (for reasons including economic growth and competitiveness). Almost all NSF's research dollars go to the university system, which the United States depends on far more than other industralized economies.

NSF expects to spend $14.5 million in fiscal 1988 on HTS—table 12. With only a few U.S. companies putting much effort into basic research, many of the preliminaries to commercialization of HTS will take place on the Nation's campuses.

Are the universities up to the job? In the short run, the answer is plainly yes. But the work of commercialization will go on for years, and as it shifts from research toward applications, a set of perennial problems in engineering research, and in university/industry relationships, could hinder the process. These problems stem from the inhospitality of universities to multidisciplinary research, and the differing goals of university and industry R&D.

Disciplinary Boundaries

Many of the Nation's universities have strong if often small HTS research efforts. As HTS technology moves ahead, multidisciplinary R&D will be essential. Progress will depend on the physics community—e.g., for theoretical guidance and an understanding of the ways in which structure, particularly at the atomic level (cystallography, flux pinning sites) determines properties (critical current densities). Chemists will add their skills, particularly in materials synthesis and characterization, as well as in processing. Materials scientists will have the job of understanding microstructural and substructural effects (grain boundaries, twins, dislocation structures), and of linking these with processing (e.g., thermal-mechanical sequences). Materials engineers will develop processing techniques that yield the needed structures (hence properties) at reasonable costs. Design of electronic devices will fall mostly to electrical engineers and physicists. Electrical and mechanical engineers will develop high-power/high-field applications—e.g., for energy storage systems. Each group has its own language, its own assumptions and preconceptions, its own world view.

To the lay person, science and technology may seem all of a piece. They are not. In private firms, multidisciplinary groups function effectively because they must—otherwise the company would not be able to compete. Over the past decade, American companies have worked hard at this, as they have faced up to the loss of technological advantages in world competition. Firms like IBM and AT&T—leaders in HTS R&D—have been seeking better ways

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[12] *NASA Technology Program Plan: High Temperature Superconductivity Technology, Preliminary Program Plan*, Vol. 1, National Aeronautics and Space Administration, Feb. 3, 1988.

Table 12.—National Science Foundation Funding for HTS R&D

	Fiscal year budget (millions of dollars)		
	1987 (actual)	1988 (estimated)	1989 (requested)
Directorate for Mathematical and Physical Sciences:			
Materials Research	$ 8.0	$10.0	$12.0
Chemistry	0.3	0.4	0.6
Physics	2.2	2.3	2.4
Engineering Directorate	1.3	1.9	2.2
	$11.7	$14.5	$17.2

NOTE: Totals may not add because of rounding.

SOURCE: National Science Foundation, 1988.

of moving new technology from the research laboratory, through development, and into production. The steady advance of technical knowledge—which inevitably entails greater specialization and fragmentation—only makes this more difficult. The job is one for management, and a continual struggle.

Universities find it even more difficult to accommodate such work, lacking the imperatives of the corporation. Specialization and fragmentation begin on campus. Indeed, disciplinary boundaries account for some of the technology gaps noted earlier in this chapter. No one undertakes needed R&D because no group of engineers or scientists looks on the problems as part of its territory (welding, wear, ceramic processing). HTS will probably face some of these kinds of problems.

NSF Centers

Federal agencies have tried to encourage interdisciplinary research in the universities, using the carrot of R&D money, but funds for programs like NSF's Engineering Research Centers (ERCs) remain small compared to those for single-investigator projects. Figure 4 shows the trends over three decades at NSF. Individual project support remains at about 70 percent of the NSF total—well above the level of the mid-1960s.[13] Still, NSF-sponsored research centers could number 80 or more by the middle 1990s, if the Foundation gets the budget increases it has been seeking.

Currently, about one-fifth of the NSF engineering budget goes for the ERCs, the first of which were established in 1985. In the Foundation's 1988 spending plan, the ERCs account for $33 million ($15 million less than NSF originally sought) of the $171 million allocated to engineering.[14]

The ERC's are relatively small and focused—e.g., on Optoelectronic Computing (University of Colorado). Annual funding levels have ranged from $1.5 million to $3.5 million. While NSF expects many proposals for HTS centers in the future, superconductivity does not fall within the purview of any of the 14 ERCs approved through the end of 1987. Indeed, this group of 14 includes only one center in the area of materials (and it is scheduled to lose its NSF support)—perhaps because the Foundation also funds about a dozen interdisciplinary Materi-

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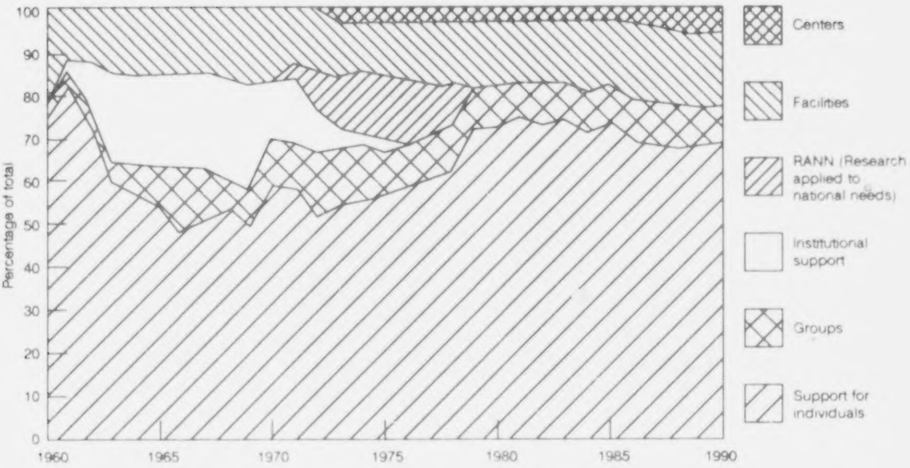
[13] About 13 percent of NSF's fiscal 1987 budget went for multidisciplinary research centers—*Department of Housing and Urban Development—Independent Agencies Appropriations for 1988*, Part 4, hearings, Subcommittee on HUD-Independent Agencies, Committee on Appropriations, U.S. House of Representatives (Washington, DC: U.S. Government Printing Office, 1987), p. 74.

[14] In the first 2 years of the program, the Foundation approved 11 ERCs (expending $27.7 million, with industry, States, and localities more than doubling the NSF contribution). Current plans call for up to 18 ERCs by the end of 1989. Under the program, NSF agrees to support centers for up to 11 years, with evaluations after 3 and 6 years. The Foundation recently announced it will discontinue support for two of the initial centers, following their 3-year reviews. For further background, see *The New Engineering Research Centers: Purposes, Goals, and Expectations* (Washington, DC: National Academy Press, 1986), and *Educating Scientists and Engineers: Grade School to Grad School* (Washington, DC: Office of Technology Assessment, June 1988), ch. 3.

On NSF's proposed S&T centers, below, see *Science and Technology Centers: Principles and Guidelines* (Washington, DC: National Academy of Sciences, 1987); also C. Norman, "NSF Centers: Yes, But . . .," *Science*, July 3, 1987, p. 21.

Figure 4.—National Science Foundation Research Support



SOURCE: National Science Foundation, 1988

als Research Laboratories (MRLs) under a separate program (see box O).

As discussed in box O, ARPA (later DARPA) —which, over the years, has financed a good deal of work in superconductivity—originally sponsored the MRLs. Five of the MRLs have moved into HTS research, with $3.5 million of NSF's 1988 support for HTS going toward these activities.

The MRLs represent an early attempt by the Federal Government to change the ground rules for university research; the ERCs, along with NSF's proposed Science and Technology (S&T) centers represent the latest. Announced by President Reagan in his 1987 State of the Union Message, the S&T centers could eventually become the largest NSF program for interdisciplinary research support. Universities submitted more than 300 proposals after this program was announced (plus a comparable number of planning proposals), a third of them in the general area of materials (and some of these on superconductivity). Given the slow growth in its budget, discussed above (table 9, Option 4), the Foundation has not yet found money for the S&T centers. In February, the Administration announced that none would be funded during the 1988 fiscal year. Instead, the Administration will seek a one-time appropriation of $150 million in fiscal 1989 to fund 10 to 15 S&T centers for 5-year periods. If Congress provides the money for these centers, it is possible that one or two of those approved by NSF might have a focus on superconductivity.

Funding for the Industry/University Cooperative Research Center program—well on the way to proving its worth—has been flat in recent years. Nor has the ERC budget grown as NSF had hoped. As discussed under **Option 9** in table 13, additional funds will be needed to expand the center programs. Growth in these programs will not have much impact on HTS unless one or more of the proposals that would focus on superconductivity wins the competition for funds. While Congress could direct NSF to launch a center specifically for HTS, this would be an unfortunate precedent, given that the Foundation has traditionally avoided tar-

Box O.—Multidisciplinary Research in American Universities

The boom in Federal R&D since the first Soviet Sputnik, launched in 1957, changed the face of American science and engineering, and the face of American universities. In 1960, the Federal Government spent about $600 million for university research; for 1988, the total will come to $6.7 billion. Yet some things have not changed. Departmental and disciplinary boundaries—separating physics from chemistry, science from engineering—remain as high as ever. Multidisciplinary research has never caught on. Although the university system has changed a great deal over the postwar period, the schools continue to train new people in old ways.

NSF's ERCs and proposed S&T centers represent a renewal of efforts to alter the patterns. Better linkages with industry have also been a goal, as with NSF funding for Industry/University Cooperative Research Centers—a long-standing interdisciplinary program in some respects more directly targeted than the other center programs, and one that has proven successful in attracting industry support.

Engineering and Science in the Universities

Since the latter part of the 19th century, engineering has moved steadily toward applied science. After World War II, American engineering schools embraced a scientific model for the profession—largely in consequence of the dominant role played by scientists in wartime developments such as radar, computing, and the atomic bomb. Few engineers had the understanding of physics, and the analytical tools of applied mathematics, needed for major contributions. Engineers were at home in factories, but not in research laboratories.

During the 1960s, Sputnik and the space race spurred further revisions of engineering courses and curricula. Added work in the engineering sciences—electrical dynamics, solid and fluid mechanics, thermodynamics—replaced design and manufacturing. Paper and pencil exercises (and computers) replaced hands-on laboratory courses. American engineers became more comfortable doing research. But many lost sight of the marketplace, and came to disdain the factory floor.

Just as important, the profession—always fragmented—remained locked into a set of disciplinary boundaries defined by 19th century technology (civil, mechanical, electrical, chemical). Each has grown steadily more specialized. Civil, mechanical, and aeronautical engineers, for instance, all deal with problems in structural analysis and design. But each has its own textbooks, and its own ways, sanctioned by tradition, of approaching similar problems. With the advent of computing and solid state electronics, electrical engineering departments dropped much of their research and teaching on electrical machinery (motors, generators)—a major point of contact with mechanical systems and mechanical engineering. New fields like control systems grew up more or less independently in departments of electrical engineering, mechanical engineering, and aerospace. Today, specialists in control system problems from electrical and mechanical engineering can hardly communicate with one another. Attempts to establish interdisciplinary departments such as engineering mechanics that could bridge the gaps often led to new entities which in turn sought to differentiate themselves. Fragmentation increased rather than diminishing.

The Government Role

Federal R&D policies contributed. As figure 4 indicated, most of the Government money goes for grants to individual investigators and small groups (usually from a single department)—a pattern that holds equally for science and engineering. This approach to research is sanctioned, not only by tradition, but by the practices of agencies such as DoD and NASA. In these agencies, mission-related technical problems are broken down and defined by agency scientists and engineers. Those parts of the problem suited for university research are farmed out to faculty members and their graduate students. Other parts of the problem go to industrial contractors, or to one of the Federal laboratories. From the point of view of the agency, this may be nothing more than good management; R&D must be tightly defined if it is to contribute. But the cumulative effect is to help shape university curricula around narrow research topics, and to maintain existing disciplinary boundaries.

NSF's ERCs and S&T centers represent renewed—not new—attempts to break down some of the disciplinary barriers in the Nation's universities.[1] A secondary objective has been to stimulate interactions with industry. Past efforts have had modest success, but have not taken hold and spread. Ironically, one of the most successful past efforts helped create still another specialized field—materials science—a field with particular significance for commercializing HTS.

The Materials Research Laboratories[2]

Rapid advances in solid-state science beginning in the 1930s laid the foundations for the field now known as materials science, a field that spans ground ranging from the the physics and chemistry of solids to metallurgy, one of the oldest of originally craft-based technologies. As the postwar explosion of science and technology continued, and the Cold War deepened, government leaders saw pressing needs for better materials. Quantum mechanics had revolutionized fundamental understanding of metals. Nylon had emerged as an engineering plastic. But university departments of metallurgy, minerals, and mining had not drawn on the new knowledge, while physicists seldom focused on practical materials problems. Federal agencies, however, had numerous needs: preventing structural failures in ships (a major problem during World War II); radiation embrittlement in nuclear reactor components; rocket nozzles that would stand up under the temperatures of blast-off, and nose cones that could survive re-entry into the Earth's atmosphere. Higher temperatures in the hot sections of jet engines would mean fighters and bombers with greater performance and range. Advances in semiconductors promised more accurate missiles, more compact and reliable avionics.

In 1960, after several years of discussion and debate within government, ARPA—then only 2 years old—announced a program of block funding for interdisciplinary materials research laboratories in the Nation's universities. As with many other shifts in U.S. science and technology policies during this period—including the establishment of ARPA itself—the MRLs probably owe their existence to Sputnik. By the end of 1961, 11 laboratories were receiving funds from ARPA. Today, there are about the same number; half a dozen have come and gone over the years.

NSF took over MRL support in 1972. ARPA found it increasingly difficult to justify hands-off block funding of university research during the Vietnam War years. The program reached its peak at the end of the 1960s, with annual budgets of nearly $19 million supporting about 600 faculty members, plus many more graduate students and technicians. (Currently, about 400 faculty get MRL support.) But ARPA's management had come to view the funding process—rolling grants, renewed each year for a period of 4 years—as untenable. They felt that block grants for a research agenda set by the universities—without meaningful DoD involvement—could not be justified either internally or to Congress, particularly after passage of the Mansfield Amendment in 1969. Most important, ARPA had little in the way of concrete R&D results to point to; the program had strengthened the infrastructure for materials research—the original goal—more than it had supported DoD missions. (American universities housed only 11 materials departments in 1964; today, there are over 50.)

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[1] On the origins of the ERCs: "We established this program to change a culture—the culture of our universities. . . . The stimulus . . . was the realization that our engineering schools were becoming increasingly science oriented and concerned with analysis of narrowly focused topics at the expense of skills involving synthesis, such as design, optimization of engineering systems, and systems integration." Again, "The ERCs were established, then, as a corrective mechanism that could help strengthen U.S. competitiveness in world markets." See N.P. Suh, "The ERCs: What We Have Learned," *Engineering Education*, October 1987, p. 16. Suh was NSF's Assistant Director for Engineering when the ERC program was set up.

[2] J.G. Ling and M.A. Hand, "Federal Funding in Materials Research," *Science*, Sept. 12, 1980, p. 1203; R.L. Sproull, "Materials Research Laboratories: The Early Years," p. 25, L.H. Schwartz, "Materials Research Laboratories: Reviewing the First Twenty-Five Years," p. 35, and H.W. Paxton, "Materials Science Research and Industry," p. 362—all in *Advancing Materials Research*, P.A. Psaras and H.D. Langford, (eds.) (Washington, DC: National Academy Press, 1987). For detailed background, see "The Advanced Research Projects Agency, 1958-1974," op. cit., especially pp. III-62 to III-65; on the later transfer from ARPA to NSF, see pp. VIII-59 to VIII-60.

Despite its name, ARPA's Interdisciplinary Laboratory Program supported materials research only. At about the same time, the AEC and NASA began funding several similar university laboratories. When NSF took the laboratories over from ARPA in 1972, the name was changed to the Materials Research Laboratory Program. Since 1985, NSF has also funded a total of 15 smaller Materials Research Groups (MRGs), again intended to be multidisciplinary, at universities without MRLs. For fiscal year 1988, the MRL Program has a budget of $28.2 million, the MRGs $7.4 million.

To the universities, the rolling block grants seemed heaven-sent. Each MRL had almost complete autonomy in distributing funds—and still does under NSF. Typically, faculty members apply to the MRL for support rather than to external agencies, conducting research within their home departments of physics, chemistry, or engineering. Originally dominated by physicists, in recent years, only about a third of MRL funds have gone to faculty associated with physics departments, another third to materials faculty, and the remainder to chemists and engineers. Evaluations have shown that MRL-sponsored research differs little from other academic research, either in quality or in multidisciplinary collaboration.

Other Precedents

NSF's ERCs and S&T centers have other predecessors. Industry/University Cooperative Research Centers (IUCRs), intended to foster long-term collaboration with the private sector, go back to 1973. IUCRs must be co-funded by industry to qualify for initial financing; they are expected to become independent of NSF in 5 years. The Foundation estimates that a fourth of the centers (11 of 44) will have successfully negotiated this transition by the end of 1988. A measure of their success: during 1987, industry and State Governments provided an estimated $22 million, far more than the $3 million NSF contribution.[3]

To encourage individual faculty members to work more closely with industry, NSF established an Industry/University Cooperative Projects Program in 1978. The program provided partial funding for projects—often running for 2 years or so—to be conducted jointly by university and industry scientists. Funding ceased in 1986.[4]

In an effort during the 1970s—much more ambitious than any of those mentioned above—, the Nixon Administration established the Research Applied to National Needs (RANN) program in an attempt to turn university scientists and engineers toward practical problems.[5] Multidisciplinary research was an explicit objective. RANN reflected the belief that applied research could help solve pressing U.S. economic and social problems. Its budget grew from $50 million in 1971 to $143 million in 1975 (Figure 4), then declined just as swiftly until the program was abolished in 1978.

The Defense Department's University Research Initiative

First funded in 1986, DoD's University Research Initiative (URI) program aims to strengthen university capabilities—teaching as well as research—needed for future defense technologies. The three services and DARPA all participate. Funding totaled $90 million in fiscal 1986, dropped to $35 million the next year, and rebounded to $110 million for 1988. Most of the URI money goes for multidisciplinary programs. DoD has solicited proposals in technical fields judged critical for defense, some of which have commercial spinoff potential. The selection process is merit-based, largely determined through DoD reviews. The Department has encouraged collaboration between universities and military laboratories, as well as with industry.

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[3] Most of the centers have been at major research universities, with large companies accounting for much of the sponsorship. While industry has provided financing, there is little evidence of close working relationships with the centers. For instance, industry and university scientists communicate only infrequently. See D.O. Gray, et al., "NSF's Industry-University Cooperative Research Centers Program and the Innovation Process: Evaluation-Based Lessons," D.O. Gray, T. Solomon, W. Hetzner, eds., *Technological Innovation: Strategies for a New Partnership* (Amsterdam: North-Holland, 1986), pp. 187-189.

[4] A survey of industry participants covering 118 completed projects found that companies had undertaken 91 follow-on projects on their own, with budgets averaging $96,000. D. Gray, E.C. Johnson, and T.R. Gildey, "Industry-University Projects and Centers: An Empirical Comparison of Two Federally Funded Models of Cooperative Science," *Evaluation Review*, vol. 10, December 1986, p. 788.

[5] *Science Policy Study Background Report No. 1: A History of Science Policy in the United States, 1940-1985*, prepared for the Task Force on Science Policy, Committee on Science and Technology, U.S. House of Representatives (Washington, DC: U.S. Government Printing Office, September 1986).

gated R&D. (Chapter 5 discusses a number of alternative approaches.)

Some academics have feared that increases in funding for centers and other multidisciplinary programs would come at the expense of single-investigator and small-group research. While a legitimate concern, figure 4 shows that the relative shift has been small. Without growth in the NSF budget, competition for limited funds will intensify. Independent research must be preserved. Even so, it would seem prudent to risk erring on the side of support for the new multidisciplinary centers, rather than on the side of a continuation of traditional funding patterns.

There are other ways as well to foster a multidisciplinary environment in the university system: for example, federally funded postdoctoral fellowships could be designed to encourage scientists and engineers planning academic careers to to move laterally into related fields—e.g., from chemistry to materials, from electrical engineering to solid state physics (**Option 10**).

Table 13.—Issue Area II: Strengthening Interactions Among Universities, Industry, and Government

Issue	Options for Congress	Advantages	Disadvantages
A. University-Industry Interactions: Multidisciplinary Research			
Commercialization of HTS requires multidisciplinary R&D. To do a better job of training people who can help American firms compete, universities will need to encourage multidisciplinary research and teaching. Federal agencies, notably NSF, have been increasing support for multidisciplinary research, but have had limited funds to accomplish this.	**OPTION 9.** Congress could: • Provide full funding for NSF to launch its proposed interdisciplinary Science and Technology centers. The Foundation seeks a one time appropriation for fiscal 1989 of $150 million to support 10 to 15 centers for 5 years. • Appropriate funds at the $5 million or above level for NSF's industry/University Cooperative Research centers over each of the next several years, ensuring that the newer centers do not overshadow this program. Congress might also consider renewed support for the Industry/University Cooperative Projects Program. Ample continuing support for NSF's Engineering Research Centers, provided evaluations indicate they are effective, also seems appropriate.	More support for multidisciplinary research and teaching could help train engineers and scientists to do a better job of bridging the gaps between research and design, development and production, R&D and marketing. Not only will this be vital for competitiveness in HTS, it is vital throughout the U.S. economy.	Without a corresponding increase in NSF's overall budget (see Option 4 in Table 9), money for centers could come at the expense of individual and small group research—one of the outstanding strengths of the American university system.
Most of the incentives in American universities reward those who pursue conventional research careers; few encourage faculty members to cross disciplinary boundaries.	**OPTION 10.** Direct NSF, along with other agencies that fund postdoctoral fellowships, to establish programs specifically for scientists and engineers who chose to move to a related field for a year or more of research.	According to the National Research Council, such fellowships "would facilitate communication among disciplines and 'seed' the faculty with individuals who are experienced in the cross-disciplinary approach."[a]	Without complementary changes in the university environment, such moves might hurt the career prospects of those accepting fellowships.
B. Government-Industry Interactions: Technology Transfer and Joint R&D			
Over the past few years, Congress has enacted several pieces of legislation intended to encourage transfer of technology from Federal laboratories to industry. These provide a framework for reform, with decentralized decision making at the laboratory level. While some of the laboratories have responded enthusiastically to the new laws, it is not clear that the agencies—especially at higher levels—have embraced this mandate.	**OPTION 11.** Conduct early oversight on the responses of major R&D agencies—particularly the Departments of Defense and Energy—to recent laws and executive branch actions aimed at speeding technology transfer and commercialization of federally funded R&D.	The oversight process could help Congress determine whether further changes in the legislative framework are needed. Matters that might be examined include: • Whether to require that Federal agencies issue regulations for implementing the provisions of the Federal Technology Transfer Act of 1986. The law does not require agencies to issue implementing regulations; indeed, it specifies that they shall not delay implementation until rules are issued. But the situation is a new one for industry too, and lack of guidelines may discourage them from approaching the laboratories.	Reforms take time to implement. It may be too early to get an accurate reading of agency responses to the new rules for technology transfer. The oversight process itself could mean that responsible officials spend time answering inquiries that otherwise would go into improving transfer processes.

[a] *Directions in Engineering Research: An Assessment of Opportunities and Needs* (Washington, DC: National Academy Press, 1987), p. 67.

Table 13.—Issue Area II: Strengthening Interactions Among Universities, Industry, and Government—Continued

Issue	Options for Congress	Advantages	Disadvantages
		• Actions taken by the laboratories to improve institutional support for technology transfer through personnel policies and provisions for royalty sharing with inventors. • Effects of agency mission on the course of technology transfer. Congress might also ask DoD and DOE how, specifically, their procedures will apply to HTS. • The success of the Federal Laboratory Consortium in living up to its mandate under the 1986 Act.	
Technology transfer may get few resources and little attention when it is not viewed as part of the agency's own mission. For HTS, effective transfer mechanisms could be especially important. DoD, with more money to spend on this technology than other agencies, has fewer reasons for working hard to transfer R&D results to commercial (non-defense) industry.	**OPTION 12:** Direct DoD, working with DOE and the Federal Laboratory Consortium for Technology Transfer, to use on a trial basis an intermediary or adjunct organization for transfer of HTS technology to non-defense firms. The intermediary would need to have well established working relationships with the private sector, and strong motives for making the transfer process function effectively.	Given DoD's funding levels for HTS R&D, transfers to the civilian side of the economy could have substantial impacts on commercialization. Once R&D results were approved for transfer by DoD, the intermediary could take on the job of working with industry, minimizing interference with the primary missions of DARPA, SDIO, and the services. Regardless of the mechanism chosen, an HTS technology transfer program could be viewed as a demonstration—with high visibility and potential relevance for other technologies.	Transfers from DoD might come to be viewed as substitutes for R&D funding by civilian agencies, to the possible detriment of commercial technology development.
Demonstration projects could help identify better methods for transferring technologies to industry, but little funding has been available. The same is true of demonstration projects involving R&D cooperation between the national laboratories and industry.	**OPTION 13:** Appropriate or allow more money to be set aside for the Federal Laboratory Consortium for Technology Transfer to undertake three or more demonstration projects on technology transfer and/or R&D cooperation over the next year or two. Projects with outcomes relevant to several agencies would be most useful. Possibilities include: • pilot programs at the State level (see Option 17 below); • development of guidelines, and trials, involving intermediary organizations (see Option 12 above); • preparation and testing of a technology transfer training program for laboratory (and industry) employees.	The FLC received about $700,000 during 1987 under a set-aside specified in Public Law 99-502, with only 5 percent available for demonstration projects. Additional funds for demonstrations—perhaps $300,000 per year—would begin to address the need.	Each technology transfer situation is unique, putting limits on the lessons to be learned. Nor can a cookbook approach to technology transfer function effectively.
If the national laboratories are to transfer technologies to industry effectively, many more laboratory employees will need to understand industrial needs and marketplace realities. While industrial (or university) scientists can arrange to work in a Federal laboratory with little difficulty, the primary need is for movement in the other direction—from the laboratories to industry.	**OPTION 14:** Authorize and encourage temporary exchanges of technical personnel (and sharing of personnel), as well as cooperative R&D projects between industry and the national laboratories. HTS could get special attention. Alternatively, Congress could create a broader exchange program to send engineers and scientists from national laboratories to private corporations for periods of 6 months to 2 years. One hundred fellowships per year would begin reaching enough laboratory employees to make a difference. Laboratory engineers and scientists could be required to work on problems of mutual interest, with the Government paying half their salaries and maintaining pension eligibility benefits.	Such a program would serve a need largely unmet—giving laboratory employees hands-on industrial experience, thereby speeding commercialization. Fellowships could be made available to laboratory personnel on a competitive basis. Temporary assignments in universities would not serve the same purpose, nor would programs that focus only on bringing industry people into the laboratories. Cost sharing by companies would help ensure that the laboratory fellows worked on commercially relevant problems.	Such a program carries risks of conflict of interest, as well as the appearance of subsidy. Moreover, the laboratories might find industry hiring away some of their more valuable people. Some firms might fear they could lose control over proprietary technology.
DOE's national laboratories are seeking a major role in helping U.S. industry commercialize HTS, but as yet have limited experience in cooperative R&D with the private sector. Working out R&D arrangements that suit industry's needs without detracting	**OPTION 15:** Direct DOE to encourage an experimental approach to cooperation with industry. As the Department's laboratories establish pilot centers for HTS R&D, and engage in other collaborative efforts with industry and universities, each center could	An experimental approach would help the laboratories learn to work with industry without consuming a disproportionate share of HTS research dollars. Trying a number of different approaches implies learning from the results, hence provision for evaluation.	Relying too heavily on cooperation between the laboratories and industry, particularly to the exclusion of other policies for speeding commercialization, would be a mistake. There is a second dan-

SBIR money has gone in the past for LTS research; Federal agencies, including DoD and DOE, are currently evaluating well over 100 SBIR proposals for HTS awards.

So far, half a dozen States have established small business innovation research programs of their own. Many States operate small business advisory services, some of which offer assistance in applying for Federal SBIR grants. Planning grants could help the States go further in complementing the Federal effort. Congress might also consider raising the Phase I ceiling from $50,000—which buys relatively little today—to, say, $100,000.

TECHNOLOGY INTERCHANGE WITH JAPAN

President Reagan's superconductivity initiative speaks of reciprocal opportunities for the United States and Japan to cooperate in R&D. The Japanese Government's pronouncements on HTS also stress international cooperation—e.g., foreign participation in government-sponsored superconductivity projects such as the New Superconductivity Materials Research Association, and the International Superconductivity Technology Center (ISTEC, ch. 3).

If nothing else, Japan has sought to respond to criticism that its research system has been closed to foreigners. But agencies of the Japanese Government also have quite concrete benefits in view—notably, new perspectives and new ideas that could strengthen Japan's capabilities in basic science. More than symbolism, international cooperation could help the Japanese reach their own objective—a more creative R&D system. The Japanese also realize that they risk loss of access to research from the United States and other countries if they do not open up their own laboratories.

New developments in HTS will continue to come from Japanese laboratories. American companies—as well as individual scientists and engineers—stand to gain from participating in cooperative projects, but only in full partnership with Japanese companies and Japanese scientists. More than direct benefits are at stake. Hands-on involvement in Japanese R&D will help Americans—as organizations and individuals—understand how the Japanese compete so effectively.

Participation and Monitoring

As this report was being completed, no U.S. firm had agreed to join one of Japan's cooperative projects as a full member, although a few had become affiliates. American companies, at this point, feel that the costs are too high—and not only the fees (full membership in ISTEC runs about $800,000). To benefit from full membership, a company would have to assign one or more highly competent professionals—fluent in Japanese—to the cooperative project. Scientists with relevant skills and experience are rare in both countries. American firms would be reluctant to send one of their best people to Japan, even if they had someone who spoke the language. (Evidently, few U.S. subsidiaries in Japan have not had much success in hiring top-rank engineers and scientists.)

For smaller U.S. firms, especially those without Japanese affiliates, any form of participation may be difficult to justify. If U.S.-based professional societies or trade associations were permitted to join Japan's government-sponsored projects, spreading the costs, American industry could gain better access to Japanese HTS R&D. Alternatively, a number of American firms might form a joint venture for such purposes as monitoring HTS R&D in Japan, keeping members aware of opportunities for individuals as well as companies, and helping transfer technology to the United States. The U.S. Government could support such an effort, perhaps by helping finance an office in Japan, or as part of a larger program such as NSF's Japan Initiative (see **Option 18** in table 14). Federal support for such an office would build on precedents including aid provided by the Commerce Department in 1984 to the American Electronics Association for a trade office in Tokyo.

Some American companies already operate Japanese affiliates primarily as listening posts, As a result of patent law changes in 1980, small businesses, non-profit organizations, and universities can gain title with relative ease to inventions they make in the course of R&D paid for by Government. In 1984, Congress extended this statutory policy to contractor-operated laboratories, including several DOE facilities. (The statutory policy does not extend to weapons laboratories, or DOE laboratories operated by large, for-profit businesses, although the Administration has initiated changes here as well.) The most recent step, the 1986 Federal Technology Transfer Act, seeks tighter links between government-operated laboratories and industry. This law:

- Provides clear authorization for government-owned and -operated laboratories to enter into cooperative R&D with private firms.
- Gives the Federal Laboratory Consortium on Technology Transfer (FLC) a statutory charter. About 400 laboratories, representing 11 agencies, belong to the FLC, which was organized to facilitate use of federally developed technologies.[15]
- Provides for agencies to return licensing income to the originating laboratory, and requires that at least 15 percent of royalties or other income go to the employees responsible.
- Directs laboratory directors to consider technology transfer activities in performance evaluations and promotions, and to include it in job descriptions.

The 1986 Act decentralizes many administrative responsibilities, giving substantial discretion to the laboratory directors. Beyond these statutory changes, President Reagan's April 1987 Executive Order 12591, on facilitating access to science and technology, establishes guidelines for all the laboratories.

While many of the laboratories have expanded their technology transfer activities over the past several years, the pace of change at the agency level has often been slow. Moreover, the discretionary authority given to laboratory directors in the 1986 Act applies only to government-operated laboratories, not to contractor-operated facilities like DOE's. To help determine whether further policy modifications might be needed, Congress could conduct oversight on the responses of the mission agencies to the 1986 Technology Transfer Act, other recent changes in the law, and to Executive Order 12591 (**Option 11**, table 13).

Transferring HTS R&D

While a new framework for technology transfer exists, it is far from clear that industry and the laboratories will be able to forge effective partnerships for commercializing technologies like HTS. Many of the formal barriers have come partway down, but the culture of these 700-plus institutions insulates them from industry and marketplace. The laboratories also differ greatly in style and tradition. Some stress engineering, others research for the sake of research. Policies with much to recommend for a DOE facility may be irrelevent for NIH, while conflicting with DoD security requirements.

Technology Transfer from DoD

Much of the Federal funding for superconductivity passes through DoD, which operates more than 70 laboratories, a pattern that will probably continue. While defense agencies work hard at transferring technologies to military contractors, diffusion to the civilian side of the economy poses special problems. These begin with the frequent requirements for secrecy, and end with the likely reluctance of the Pentagon to accept such a burden as a major ongoing responsibility.

In authorizing an HTS program for fiscal years 1988 and 1989, Congress instructed DoD (and DOE, when its laboratories receive DoD funds) to give special attention to transfers of technology to the private sector.[16] Apparently,

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[15] "Strategic Plan: 1988-1992," Federal Laboratory Consortium for Technology Transfer Administrator, Fresno, CA, October 1987.

[16] Section 218 of the National Defense Authorization Act for 1988 and 1989 (Public Law 100-180) earmarked $60.56 million annually for 2 years for a DoD program on HTS. Congress appropriated only $15 million for fiscal 1988 under this provision, which went to DARPA for initial funding of its processing R&D effort, described earlier in the chapter.

DoD intends to use existing mechanisms to implement the requirements of the Defense Authorization Act, rather than establish procedures specifically for HTS. While current practices may suffice for transferring HTS technologies to defense industries, they will probably be less effective for transfers to firms on the civilian side of the economy. Instead, it might make sense to assign the task of working with non-defense firms to an intermediary organization (**Option 12**).

A number of arrangements seem feasible. Several DOE laboratories—including Argonne, Oak Ridge, and Ames—have set up adjunct organizations to handle technology transfer. DoD could contract directly with an existing organization—e.g., a not-for-profit R&D laboratory like Battelle. An intermediary charged exclusively with transferring technical knowledge to commercial enterprises could play a useful role during the stages of technology development and commercialization processes that are not germane to DoD's mission.

Demonstration and Evaluation

Technology transfer has significance going beyond HTS. So does cooperation in R&D between industry and the national laboratories. But both in the laboratories and at middle and upper ranks in the agencies, commitment to meaningful change has not always been visible. Information about what works would help; successful demonstration projects could have considerable impact (**Option 13**). The FLC has the authority to conduct demonstrations, but its set-aside funds from the agencies paid for only one such project during 1987.

Making technology transfer function effectively and efficiently will demand systematic, empirically-based analysis of transfer processes (including cooperative R&D), and of subsequent impacts on innovation and commercialization. Demonstration projects without critical evaluation of results may not accomplish much.

Laboratory Personnel

People transfer technology much more effectively in person than through reports, and they do so best when they work together (rather than in meetings). Transferring HTS technologies from the national laboratories means: 1) bringing people from industry into the laboratories to work on HTS, perhaps through cooperative R&D projects; and 2) sending people from the laboratories to industry so they can learn what commercialization is all about. In the short run, the first of these steps has much to offer for HTS. The second step is necessary for lasting changes in the culture of the laboratories, and for long-run success in better integrating the laboratories into the Nation's R&D infrastructure. Because of possible conflicts with DoD missions, personnel exchanges have greater potential attraction at DOE (non-weapons) laboratories.

The laboratory system attracts many highly competent people with more interest in research than in the practical problems of industry—no surprise, given that commercialization has not been a mission of the laboratories. Some people join a laboratory precisely because they have no wish to work on industrial problems. They may be highly capable professionals, dedicated to research, but even if motivated to work with industry, laboratory employees may not know how—through lack of exposure to corporate life and the realities of the marketplace.

Agency policies have been broadened in recent years, so that many Federal employees can do consulting (on their own time), or take leaves of absence to work in industry. Both these steps will make a difference. So could a program of temporary appointments sending laboratory personnel to the private sector (**Option 14**). Although industry employees can come to the laboratories quite easily, flow in the other direction will have more impact in changing the laboratory culture. Congress could explicitly authorize and encourage fellowships and/or exchanges. Several of the HTS bills introduced in the 100th Congress authorize industrial fellowships at Federal laboratories, but not fellowships that would send laboratory employees to industry. Others have tied personnel exchanges to cooperative R&D programs. The need is a broad one: there seems no necessary reason to tie personnel exchanges either to cooperative R&D or to HTS.[17]

Cooperative R&D

Cooperative research bringing together laboratory employees with those from industry—and perhaps from universities—offers another way to integrate the laboratories more effectively into the Nation's technological infrastructure. The possible arrangements serve needs ranging from efficiency—avoiding too much duplication—to lengthening industry's time horizons, as discussed in the next chapter (see box R on collaborative R&D). The discussion below focuses on HTS—e.g., approaches such as the proposed National Laboratory Cooperative Research Initiative Act, S. 1480 in the 100th Congress.[18]

DOE has itself moved toward closer cooperation with industry. In April 1988, the Secretary of Energy designated three national laboratories—Los Alamos, Argonne, and Oak Ridge—as superconductivity pilot centers. The pilot center approach, including expedited procedures for contracting and project approval, and transfer of intellectual property rights, had been initially proposed by Los Alamos, which had been asked by the Secretary to explore mechanisms for cooperative ventures.

DOE user facilities have begun attracting the attention of private firms: the Department's figures show 1600 industrial visits in 1987, compared with 260 in 1981.[19] But collaborative projects with industry, though on the rise, involved just 57 companies and R&D valued at about $110 million during 1987 (for the multiprogram laboratories). Given this so-far modest showing, Congress might direct DOE to take an explicitly experimental approach to cooperation with industry (**Option 15**). Rather than a full-scale effort, structured trials could help industry and the laboratories find ways of working together while avoiding unrealistic expectations and the danger of steering too many HTS R&D dollars to untested programs. Pilot projects could take a variety of forms: firms might work with the laboratories singly or in groups; potential rivals could choose to pursue pre-competitive projects jointly; firms with similar R&D objectives, though in different businesses, could cooperate, along with those having supplier-customer relationships.

Industry will have to take much of the initiative if the DOE laboratories are to aid in commercialization of HTS. Companies must be willing to search out areas of expertise in the laboratory system, and seek to take advantage of them—contributing a substantial share of project funds. If Federal dollars cover too high a fraction, the company may no longer feel it has a stake in outcomes; projects can stray from the needs of commercial technology development. The laboratories might also find that the only companies working with them were those with few prospects for commercial success, and little choice but to take whatever help DOE might offer.

At the same time, while few firms are likely to make substantial financial commitments without guarantees of influence over research goals, industry cannot have too much control, else planning horizons will shorten: unless cooperative projects have riskier and/or more generic R&D objectives than companies would pursue on their own, there is little justification for Government participation. These considerations suggest projects that last 3 to 5 years or more, with industry cost sharing in the range of 40 to 60 percent.

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[17] The President's Commission on Executive Exchange, in response to the April 1987 Executive Order, has been working on a small-scale plan for exchanges of technical personnel between industry and Federal laboratories. Industrial participants will probably be limited to relatively large companies that can afford to share the administrative expenses, as well as picking up part of the costs of the exchange. Contractor-operated laboratories may not be covered—thus excluding most DOE facilities.

[18] Introduced in 1987, S. 1480 also includes provisions for cooperative R&D on mapping the human genome and semiconductor manufacturing. The bill, in its original and modified versions (Senate Amendment 1627, introduced in March 1988) would direct the Secretary of Energy to establish cooperative centers at DOE national laboratories for HTS R&D, and give the laboratories greater autonomy in negotiating agreements with private companies and universities.

[19] The 1987 estimate comes from DOE's Laboratory Management Division, that for 1981 from the statement of Dr. James Decker at the Joint Hearing on Technology Transfer before the House Committee on Science and Technology and the Subcommittee on Energy Research and Development of the Senate Energy and Natural Resources Committee, Sept. 4, 1986 (Washington, DC: U.S. Government Printing Office, 1987), p. 22.

It might also be appropriate for Federal agencies to share costs with industry-based collaborative R&D ventures (**Option 16**—also see ch. 5). The justification? Longer time horizons. While national laboratory participation might sometimes be desirable, there seems no reason to make this a precondition.

Regardless of final policy decisions on cooperative R&D, mechanisms for evaluating differing approaches, and disseminating the results—and not just the success stories—will be needed. The approaches emerging could have relevance going well beyond superconductivity.

State Programs and Approaches

Over the past decade, many States, in the name of economic development, have established programs for supporting high-technology businesses. Some already support HTS.

Among the more visible initiatives:

- advanced technology centers intended to attract and work with high-technology industry;
- centers of excellence at state-supported universities (several—e.g., at the University of Houston and the State University of New York at Buffalo—have been established in superconductivity);
- small business innovation research programs, patterned after those at the Federal level (adopted quite recently by half a dozen States);
- technology extension services, intended to help companies attack technical problems and diffuse know-how to industry;
- financial assistance for start-up firms and small businesses.

Many State governments have also established advisory commissions and councils on science and technology.

The variety and innovative nature of State programs also mean that some of the undertakings have been fragmentary. Few States have comprehensive efforts. The New York State Science and Technology Foundation, and Pennsylvania's Ben Franklin Partnership, have been among the more extensive. Still, State governments have some tools to call upon for aiding HTS technology transfer and commercialization that are not available to the Federal Government. They also have compelling motivations—jobs and income.

OTA has previously suggested that Federal matching funds for State programs such as technology extension services could be appropriate.[29] Additional possibilities (**Option 17**) include small planning grants to the States for strengthening R&D-based economic development initiatives. Pilot projects might include a demonstration program for State technology extension services, as provided for in S. 907 (incorporated in the omnibus trade bill passed by Congress but vetoed by President Reagan in May 1988).

This option could complement existing Federal Small Business Innovation Research (SBIR) programs. Under the Small Business Innovation Development Act of 1982, Federal agencies must allocate 1.25 percent of extramural R&D budgets exceeding $100 million for SBIR awards:

- Phase I contracts provide up to $50,000 for demonstrating the merit of an idea.
- Under Phase II, agencies may award up to $500,000 for taking Phase I concepts to the pre-prototype stage.
- In Phase III, companies can proceed with development using non-federal funds, or seek non-SBIR money from Federal agencies.

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[29] "Development and Diffusion of Commercial Technologies: Should the Federal Government Redefine Its Role?" staff memorandum, Office of Technology Assessment, Washington, DC, March 1984, pp. 10 and 48.

Precedents for assistance to the States include Federal funding during the 1960s and early 1970s for strengthening State and local capacities in dealing with issues of technology and science. At the time, these efforts were primarily focused on "public technology"—e.g., the direct needs of State and local governments, rather than economic development and business needs.

Table 13.—Issue Area II: Strengthening Interactions Among Universities, Industry, and Government—Continued

Issue	Options for Congress	Advantages	Disadvantages
from broader laboratory missions could require considerable experimentation.	be designed somewhat differently, even though all were charged with aiding commercialization.	To succeed, the laboratories will have to be self-critical. Approaches that worked for HTS could be adopted elsewhere.	ger as well. DOE and the laboratories might find it difficult to shut down cooperative projects that proved ineffective, or were no longer needed.
Under the right circumstances, consortia for R&D—involving several private firms in precompetitive projects—could be an efficient mechanism for building the HTS technology base. Yet the time horizons of industry consortia are unlikely to be that much longer than those of individual firms.	**OPTION 16:** The Federal Government could make funds for HTS R&D available on a cost-sharing basis to industry consortia, provided the funding agency determines that public money will serve to extend the R&D time horizon.	Cost sharing of longer term R&D would address a critical problem for U.S. competitiveness. The Federal contribution could involve provision of facilities (e.g., at a national laboratory) and of temporary assignments of personnel to a consortium, in addition to financing.	Any project involving Federal funding would be subject to the vagaries of the budget process. Unless Government, as well as industry, lengthened its time horizons, money could be wasted. On the other hand, cost-sharing, once started, might be difficult to stop—even if, in time, the justification vanished.
State Governments have a broad range of economic development tools at their disposal, in addition to the direct funding for R&D that some have provided. States could help commercialize HTS through programs that accelerate the diffusion of research results to industry. At present, however, linkages between State Governments and national laboratories within their borders tend to be ad hoc and not very well established.	**OPTION 17:** Congress could: • Provide small planning grants to the States for strengthening R&D-based economic development initiatives, including grants for the evaluation of existing programs. It may take 5 years or more for States to put new programs in place; planning grants available now could mean better capabilities in the States by, or when HTS technologies begin moving out of the laboratory. • Fund several State Government pilot projects embodying different approaches to the transfer and commercialization of federally funded HTS R&D conducted in universities as well as national laboratories. • Direct Federal agencies to give greater weight to support from State Governments in evaluating proposals for university-based R&D centers, and other programs where commercialization is a major objective.	Strengthened capacities in the States to assist smaller businesses in commercializing innovative technologies would complement Federal SBIR (Small Business Innovation Research) programs, particularly Phase II efforts. Planning grants could also help the States find ways of bridging the gap between Phase II and Phase III awards.	Few State programs have been evaluated by independent parties; little is known about the approaches that work best. Federal assistance could end up favoring States that might need help the least—e.g., those that already have well-developed programs.

SOURCE: Office of Technology Assessment, 1988.

TECHNOLOGY TRANSFER: THE FEDERAL LABORATORIES

Much of the Federal funding for HTS R&D is going to government laboratories—mostly facilities run by DoD and DOE, but also to NBS and NASA research centers. These laboratories differ in missions, in their historical ties with industry, and in operating arrangements. While NBS has long had good relations with industry, and DoD laboratories often work closely with military contractors, few Federal laboratories have accomplished much in commercialization. This has not, after all, been one of their tasks. *Whether the laboratory system will be able to contribute much beyond a general strengthening of the HTS technology base remains an open question.* Certainly it would be a mistake to rely heavily on the laboratories for commercialization until they have proven themselves.

New Rules for the Laboratories

For many years—as congressional hearings and an accumulation of studies pointed to the large fraction (said to be 90 percent) of federally owned patents never licensed or otherwise commercialized—the U.S. Government has sought to stimulate commercial use of publicly funded R&D. Since 1980, Congress has enacted a series of laws intended to give industry greater access to the laboratory system, and to speed transfers of technology to the private sector.

Table 14.—Issue Area III: Technology Interchange with Japan

Issue	Options for Congress	Advantages	Disadvantages
Smaller U.S. companies may not have the resources to keep up with fast-breaking developments in Japan. While the Japanese have offered foreign firms opportunities to participate in government-sponsored cooperative projects, the response to date has been tepid. A joint venture or an organization such as a professional society or trade association might be able to spread the costs and help American industry gain access to Japanese HTS R&D.	**OPTION 18.** Provide a seed grant to a professional society or trade association for an office in Japan to monitor developments in HTS, with funds sufficient to operate the office for perhaps 5 years. A non-profit organization such as the American Institute of Physics, the American Chemical Society, or the Federation of Materials Societies should be an acceptable vehicle in Japanese eyes.	One way or another, the United States should take the Japanese up on their offers to cooperate in HTS research. In addition to serving as liaison to forums like the New Superconductivity Materials Research Association, a professional society or trade organization could help screen the latest scientific and technical information in Japanese, identifying HTS research reports for translation and distribution. This function could complement the current effort, quite small, by the Commerce Department under the Japanese Technical Literature Act (see Option 20 below).	Japan might gain more than the United States from cooperation in HTS. Much of the U.S. work will [illegible] then, where it will be [illegible] of Japan's work will [illegible] this information.
Few technical professionals in the United States have the language skills or inclination to take temporary appointments in Japanese laboratories—the most direct means for transferring technology and know-how from Japan to the United States, and a necessary step in improving American understanding of Japan's research system.	**OPTION 19.** Monitor progress in implementing NSF's Japan Initiative, appropriating additional money if U.S. funds—together with contributions from Japan—cannot sustain all components of the program.	To take advantage of R&D opportunities and Japanese technical know-how, more Americans need not only language training, but experience working in Japan.	Sending more engineers and scientists to Japan, and further language training for professionals, will [illegible] want. [illegible] guage training in U.S. primary and secondary schools.
No more than a tiny fraction of U.S. scientists and engineers will learn Japanese in the near future, leaving an ongoing need for prompt translations of Japanese scientific, technical, and business publications—including informally circulated "gray literature."	**OPTION 20.** Direct the Department of Commerce, as part of its responsibilities under the Japanese Technical Literature Act of 1986 (Public Law 99-382), to establish a program specifically for gathering, evaluating, and disseminating information on Japanese science/technology and business activities as they relate to HTS. The effort might be viewed as a model for improving the effectiveness of Commerce's programs on Japanese information. For insightful evaluations of technical efforts in Japan, Commerce will probably need help from Federal agencies with greater expertise in engineering and science.	Access to foreign scientific and technical information has become increasingly important as U.S. technological advantages have diminished. In the past, technical translations from German and Russian have been more common than from Japanese.	Translation and technical evaluation will need substantial and higher funding levels to accomplish [illegible] Commerce appropriated $[illegible] to implement the Japanese Technical Literature Act; in 1988, the Department [illegible] to reprogram $[illegible] for this purpose. An aggressive effort on HTS alone would consume [illegible] ing and evaluation of technical information is particularly important, and expensive. To this point, U.S. companies have shown little interest in Japanese technical and scientific literature.

SOURCE: Office of Technology Assessment, 1988.

just as the Japanese maintain technology centers in the United States. The Japanese should be willing to allow group participation by American firms in information-exchange activities such as the New Superconductivity Materials Research Association. Moreover, there is no reason why a corporation set up as a joint venture should not qualify to join ISTEC.

Language Training; Fellowships in Japanese Laboratories

If the United States is to make better use of R&D conducted in other countries, more Americans will have to seek and take temporary assignments in foreign laboratories. At present, few U.S. engineers or scientists have the right combination of technical qualifications, language skills, and motivation to work in Japan (in part because they may feel that their employer—and the U.S. labor market as a whole—will not reward them for learning Japanese and spending time there). As many as 7,000 Japanese engineers and scientists are currently at work in the United States—in government facilities, as well as universities; perhaps 500 Americans work in Japanese laboratories.[21]

Japan offers research fellowships for foreigners under the sponsorship of its Key Technology Center, as well as the Ministries of Foreign Affairs and Education. Given the paucity of language skills within the U.S. technical community—and such less tangible but no less substantial barriers as difficulty in finding employment for husbands or wives—not many Americans have sought out these opportunities. Cultural differences and high living costs create obstacles particularly for more senior American engineers and scientists, including those in R&D management positions.

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[21] E. Lachica, "U.S., Japanese Negotiators Deadlocked on Tapping Each Other's Technology," *Wall Street Journal*, Jan. 22, 1988.

NSF's Japan Initiative, launched in 1988 with an allocation of $800,000 from the Foundation's budget for bilateral programs, is designed to encourage more Americans to take advantage of research opportunities in Japan. The program offers fellowships for language training, as well as financial support while in Japan. Japan's Prime Minister Takeshita, moreover, has announced that that his government will give NSF $4.8 million to finance work in Japan by U.S. researchers. With this offer, near-term funding for the Japan Initiative seems adequate. NSF is seeking $1.6 million for the program in fiscal year 1989; Congress might monitor progress in implementing the program, and appropriate additional money if U.S. funds (together with contributions from Japan) cannot sustain all its elements (**Option 19**). Congress might also wish to consider greater Federal assistance to American schools and universities for language training.

Technical Information

Most U.S. scientists and engineers necessarily will continue to rely upon translations for technical information from Japan. Congress could direct the Commerce Department to expand its small program for translations of Japanese technical literature under the Japanese Technical Literature Act, perhaps appropriating funds specifically for information on HTS (**Option 20**). A professional society or trade organization (as discussed under Option 18), in addition to serving as liaison to organizations like the New Superconducting Materials Forum, could help screen the latest scientific and technical information on HTS.

While major scientific findings from Japanese laboratories normally see publication in English or another Western language, less of the Japanese engineering literature is translated. Moreover, Japan produces a large volume of "gray literature"—company, university, and government reports, as well as other informal documents not widely circulated. The gray literature, hard to acquire outside Japan, often includes important technical and business information.

Foreign Access to U.S. Technology

The U.S. Government has signaled Japan and other nations that it may restrict outflows of information from Federal HTS research. The July 1987 Federal conference, at which President Reagan announced his superconductivity initiative, was itself off-limits to representatives of foreign governments. In the private sector, the Council on Superconductivity for American Competitiveness limits its membership to U.S. corporations and citizens. One title of the Administration's proposed Superconductivity Competitiveness Act, sent to Congress in February 1988, would permit agencies to withold scientific and technical information requested under the Freedom of Information Act under some circumstances.

The scientific community has long argued that restrictions on information exchange harm its enterprise, and can only be justified on strict grounds of national security. But the question for HTS is rather different. Proposals such as the Administration's seem to assume that the United States is far enough ahead in HTS to have something to protect. OTA has found no evidence supporting such an assumption. *Lacking a decisive lead in the R&D race, measures seeking an equitable two-way flow with countries such as Japan have much more to recommend them.* Of course, the threat of embargoes on scientific and technical information helps keep the pressure on other nations to provide access to their own research systems (box P).

Box P.—Reciprocity in Flows of Technology and Information

Under the Technology Transfer Act of 1986, Federal laboratories are to take reciprocity into account before entering cooperative R&D arrangements and licensing agreements with foreign-owned enterprises (they are also to give preference to U.S. businesses). Likewise, under President Reagan's Executive Order on access to science and technology, agencies are to consider whether the foreign firm or country not only permits, but encourages, U.S. organizations to enter into comparable R&D arrangements or licensing agreements. The order directs agency heads to consult with the U.S. Trade Representative (USTR) before entering into such agreements; as this report was being completed, USTR was preparing guidelines for the national laboratories on implementation of the Executive Order.

But what does "reciprocity" mean for research systems as different as those of Japan and the United States? Much of the best research in Japan takes place in industrial laboratories, where it is not only largely inaccessible to outsiders, but beyond the reach of policies that the two governments might agree on. In the United States, heavy reliance on university research, a tradition of openness, and the fact that English has become the universal language of science, mean that almost anyone can tap into many of the results. For such reasons, the U.S. National Academy of Sciences has begun to explore "symmetrical access." The goal is to find ways of specifying scientific and technical knowledge having equivalent value for transfer between nations. The NAS effort could help create a framework for policy development.

CONCLUDING REMARKS

In the months following the initial breakthroughs in superconductivity, the U.S. Government moved quickly to redirect R&D support. The growth in funding—from virtually nothing in fiscal 1986, to $45 million in 1987, and $95 million in fiscal 1988—demonstrates the responsiveness of the system. Yet there are real reasons for concern: lack of new money for HTS; the possibility of a reaction against continuing high levels of R&D spending unless exciting new results keep coming in; heavy *de facto* reliance on DoD and DOE to generate technology that industry can commercialize. Budget uncertainties in the R&D agencies, which lasted well into the current fiscal year, put many federally funded projects on hold, slowing U.S. progress.

Defense-related spending—centerpiece of the Federal R&D budget since World War II—leads to major new commercial products or processes less frequently than in earlier years. The reasons include a drop in support for both generic and high-risk, long-term R&D relative to the overall DoD R&D budget, as well as growing isolation of the defense sector of the economy. Meanwhile, funds for applied research that would fill the gap between basic science and the short-term projects conducted by industry have been cut back: the U.S. Government spends little money on work that would strengthen the foundations for commercial industries.

At present, DoD has roughly half the Federal money for HTS. The field is new, still in the research stages. Much DoD-sponsored R&D over the next few years should yield broadly useful results. Thus DoD's ample resources could become a major asset in commercializing HTS. But the Pentagon will begin steering dollars to support mission-specific applications as soon as these are in view—indeed, may already be doing so.

When it comes to the Department of Energy, which is getting 30 percent of the Federal funds, the primary questions concern the ability of the national laboratories to forge new cooperative relations with industry. The laboratories are changing. But the system is a big one, burdened with inertia; commercialization has not been a significant mission. It will take a major departure from business as usual for the laboratories to have much impact on commercialization of

HTS (which is not to say that when the next set of opportunities comes along, the DOE laboratory system may not be in a better position to respond).

The Federal Government pays for about two-thirds of the R&D carried out on the Nation's campuses, chiefly through awards to individual faculty members. Given the short-term orientation of most business-funded R&D, the NSF budget for HTS is particularly critical. Policies aimed at breaking down some of the disciplinary barriers in American universities, and creating environments where truly interdisciplinary research could flourish, would help broadly in the commercialization of this and other technologies.

The R&D budget is not the whole of technology policy. Nor is technology the only ingredient in successful commercialization. All of the policy options covered in this chapter, taken together, would be no more than a first step in addressing the competitive difficulties of American industry. Still, the money Federal agencies spend on R&D, the ways they spend it, their efforts to transfer technologies to industry—actions taken every day by more than a dozen agencies—have enormous long-run impacts. When companies search for competitive advantages through proprietary technology, they draw continually on this publicly funded, publicly available technology base. More effective interactions among the major players in the R&D system—industry, the national laboratories, universities—would speed the generation of technical knowledge, and, perhaps more importantly, its use.

In a time of budgetary stringencies, mechanisms for establishing R&D priorities across the agencies become more critical than ever. This is perhaps the single most important point raised in this chapter. To maintain its competitiveness, the United States must generate today the technical knowledge that industry will depend on tomorrow. For HTS, this means, not only effective mechanisms for setting R&D priorities, but stability and continuity in funding. These needs imply another: a strategic view of the ways in which federally funded R&D can spur economic growth and competitiveness—the subject of the next chapter.

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Chapter 5

Strategies for Commercial Technology Development: High-Temperature Superconductivity and Beyond

SUMMARY

Together, a collection of government actions constitutes a strategy, just as the actions of a corporation's upper management constitute a strategy. *De facto* strategies, though hardly unheard of in business, are more common in government. Indeed, to some, the very notion of strategy implies a measure of loss in one of the primary strengths of U.S. technology policies—the flexibility of Federal agencies, their ability to respond quickly to new circumstances.

Regardless of approach for promoting commercial development of high-temperature superconductivity (HTS)—and regardless of whether the approach is called a strategy—success will require *diversity* in sources of funding and in the R&D programs that Government money supports. Earlier chapters stressed the uncertainty in prospects for HTS. With several agencies involved, good ideas will get a hearing—at the Defense Advanced Research Projects Agency (DARPA), if not the Department of Energy (DOE) or the National Science Foundation (NSF). Duplication, in any case unavoidable, can spur competition. *Continuity*, likewise, will be important. To encourage U.S. industry to take a longer view, the Federal Government must do so itself (a need addressed by several of the policy options in ch. 4).

In keeping with continuity and stability in funding, any strategy for HTS should *avoid high visibility*. If policymakers or the public look to Federal programs for near-term breakthroughs, disillusionment will follow. Technological advance cannot keep up with expectations fed by the media.

The Federal Government will have to *let market forces drive HTS technology* as much as possible. Historically, governments have done a poor job of trying to anticipate what markets will demand, if not the course of technological evolution; picking R&D fields ripe for major technical advance is one thing, picking winning commercial applications quite another. Policies that pull technologies into the marketplace in more subtle ways—e.g., through government procurement—can work, especially in conjunction with R&D funding designed to push the technology along. *Market pull coupled with technology push*—in a policy environment that encourages *collaboration among industry, universities, and Government*—will help speed commercial technology development. Box Q discusses these and other operating principles in more detail.

This chapter considers three approaches through which the Federal Government might foster commercialization of HTS:

- *Flexible response*. Strategy 1—the current, *de facto* U.S. policy—grows naturally out of postwar U.S. technology policy. Characterized by strong support for basic science and for mission-oriented technology development, direct measures for supporting commercial technology development find little place.
- *An aggressive response*. Strategy 2, would differ in three major ways from current policies. First, NSF would have more money for HTS—in essence, an insurance policy to make certain that good ideas for basic research have a shot at funding. Second, the Federal Government would share in the costs of private sector collaborative R&D ventures. The rationale: more work within industry on long-term, high-risk HTS R&D. Third, a working group of experts from industry, universities, and government would be assembled to decide which collaborative R&D proposals were worthy of support, and to otherwise advise on policy measures.

Box Q.—Strategy: Key Ingredients

Disagreements over strategies for technology development reflect differences of opinion over technical questions (How long will it take to develop flexible conductors made from the new superconductors?) and over market conditions (What applications will be most attractive at liquid nitrogen temperatures?), as well as over matters of political preference (Is it proper for government to support commercial technology development directly?). Some policies—e.g., support for basic research—find nearly universal support. So do some of the other ingredients that would find a place in almost any strategy for supporting HTS:

- *Diversity* in sources of R&D support. No one knows what new discoveries may emerge in superconductivity, where they will come from, or when. A portfolio of research makes more sense than one or a few centers of excellence; so do multiple sources for contracts and grants.
- *Continuity* in support, over a period that could easily be a decade (as emphasized in ch. 4).
- A judicious *balance of technology push and market pull*. Technology push via R&D support works best when accompanied by policies such as government purchases and demonstration projects that help pull high-risk, high-cost technologies into the marketplace. Overall, however, government should *let industry drive technology as much as possible*.
- *Measures that encourage collaboration among universities, industry, and Government.*

Diversity

Many sources of support, and many centers for R&D, may mean duplication of effort, but that is not necessarily bad. Overlap breeds competition and helps ensure that no path goes unexplored. There is another side to diversity. Accountability can suffer: if everyone is responsible for HTS, then no one is fully accountable. Still, lacking an overriding goal—sending a spacecraft to Mars, or building a magnetically-levitated (maglev) rail system along the Eastern seaboard—where is the need for centralized responsibility? Development of HTS is a broad objective, and also a fuzzy one: the technology cuts across the missions of a number of agencies.

NSF funds research proposals rated highly on grounds of their promise in advancing science and technology; the subject of the research carries less weight. Not without controversy, the process nonetheless has found wide acceptance. But when it comes to projects in the mission agencies, decisionmakers do not always see eye-to-eye on what should be supported. In the early days of computer technology, visionary projects such as Whirlwind and ILLIAC were as fiercely opposed by one set of agencies as they were favored by those paying the bills; U.S. computer technology would not have advanced so quickly had any one agency been solely responsible.[1]

Continuity

In reality, diversity is seldom a problem in the decentralized U.S. system—it comes naturally. The more common problem is continuity. Stop-and-go decisions have bedeviled U.S. technology policies, as chapter 4 makes clear. Congress passes the Stevenson-Wydler Act, but a new Administration does not fully implement it. The executive branch seeks to double the NSF budget over five years, but Congress does not appropriate the funds.

If there is a secret to Japanese technology policy, it lies in continuity—stability in commitment and financing, without rigidity. Publicly-funded R&D programs in Japan, many of which have enhanced the competitiveness of Japanese industry although budget levels have been modest, often begin with an 8- or 10-year planning horizon. R&D priorities change over the course of the maglev train program (ch. 3) or the fifth-generation computer project as results come in and new directions open. Budgets may change too. But sharp reversals are rare. A decade-long time horizon stands for all to see as a demonstration of commitment by both government and industry. R&D sponsored by the U.S. Government often lives from one budget cycle to the next; the consistency seen in Japanese policies has few precedents, even in defense (just as few American firms have shown the persistence in product development that led to export successes like video-cassette recorders for the Japanese).

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[1] "Government's Role in Computers and Superconductors," prepared for OTA by K. Flamm under contract No. H36470, March 1986, pp. 9-27.

In the United States, the spectacular success of a few flagship efforts like the Manhattan Project and Apollo left a trail of unrealistic expectations. NSF's RANN program (box O, ch. 4) sprang from the notion that the technical expertise needed to put men on the moon could be turned, almost as directly, to social problems. Operation Breakthrough, the Department of Housing and Urban Development's effort to revolutionize the technology of residential construction, grew from the same soil. RANN came and went in half a dozen years, Operation Breakthrough even more quickly.

HTS will be equally vulnerable. The public's expectations have been raised by a year and a half of scientific discoveries, a Federal conference featuring the President and three cabinet officers, and a Nobel Prize—all accompanied by ample media coverage. In the absence of steady progress, public support may wane; the painstaking and laborious work needed to turn science into useful technology spawns few headlines.

Low Visibility

In the United States, publicly-supported R&D programs have generally been more successful, and more durable, when they avoid high visibility.[3] Apollo was the exception, not the rule. If policymakers or the public look to a Federal HTS program for immediate technological or commercial triumphs—in the extreme, as a flagship in the international competitive struggle—they will be disappointed. A high-visibility, crisis-driven program such as the synthetic fuels initiative of the 1970s may collapse on itself, and in doing so harm the cause of future support for related efforts.

Technology Push and Market Pull

Visibility, by itself, did not do in the synthetic fuels program. The failure lay in attempts by government bodies to anticipate the course of technological evolution and the needs of the marketplace. (Japan has not been immune: the Ministry of International Trade and Industry tried to force Honda out of the auto business in the 1960s.) The lesson, repeated many times over: in the absence of convincing reasons for doing otherwise, let market forces drive technology.

Although picking winners is something that Federal agencies have never done well, policies that serve to pull the market in more subtle ways have proved beneficial. They are particularly effective in conjunction with R&D funding designed to push the technology along.

To elaborate, the Federal government has confined its role in (non-defense) technology development largely to funding research, on the assumption that the commercial market could and would create the necessary demand for resulting products. But this is an area where the market does not function perfectly. Because product development efforts in high-risk technologies are extremely expensive—accounting for nearly 80 percent of R&D costs—firms must have some confidence that there are customers at the end of the tunnel; potential customers often do not have enough information or certainty to provide that assurance, however. Thus, there is a role for government in helping assure an early market for such products.

In computers and semiconductors, Federal Government procurement provided that assurance. The defense-space share of the total computer hardware market was 100 percent in 1954, and it exceeded 50 percent until 1962. Similarly, during the early years of integrated circuit production, defense and space procurement accounted for almost 100 percent of sales. Given assurances of stable—indeed growing—demand, companies raised their own R&D spending. Technological advances coupled with learning and scale economies led to dramatic price reductions for computers. Even more important was the "demonstration effect": successful use of computers by military and space agencies proved their value to a skeptical business community.

Defense procurement was effective at pulling computer and semiconductor technologies along for two reasons. The government's mission-based needs meant that agencies evaluated technological alternatives carefully and provided valuable user feedback to suppliers. Moreover, agency needs, and the technologies they spawned, meshed closely with business needs. (As one computer executive observed, "Space and defense computer applications . . . served as a 'crystal ball' for predicting the future direction of computer use in industry.") Federal demonstration projects have been similarly effective under the same conditions, i.e., when mission-based government support of a developing technology steers it in a direction which converges with commercial interests.

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[3] "Collaborative Research: An Assessment of Its Potential Role in the Development of High Temperature Superconductivity," prepared for OTA by D.C. Mowery under contract No. H36730, January 1988, pp. 13-14 and 67-68.

Lacking this, though, the synergy can quickly vanish. Because of the growing divergence between military and civilian technologies and markets, defense procurement no longer has the positive impact it once did on commercial technology development. That issue aside, procurement and demonstration projects have been less effective when they have been done without the guidance of an agency mission.

Governments can strengthen market forces in other ways: in Japan, government-financed enterprises buy computers and robots and lease them to end users. The result? Guaranteed markets for the manufacturers, and reduced risk for the customers, who can turn back the equipment if they find it unsatisfactory.

Finally, government regulations can also pull technologies into the market, sometimes with good results. Federal fuel economy standards created incentives for American automakers to improve their capabilities in engineering and producing small cars. Technical standards (e.g., for computer languages) can be an important spur to technology diffusion.[8]

As this discussion suggests, market pull policies create vexing dilemmas for governments. Policies to support the adoption of publicly-funded R&D results are an essential component of a government effort to develop technology. But insofar as these incentives for technology adoption target specific applications, policymakers are placed in the position of trying to forecast the course of technological evolution and anticipate the commercial market. This is a task they have done poorly in the past.

Interactions Among Industry, Universities, and Government

A final principle, again emerging from the postwar history of high technology: collaborative interactions among universities and industry speed technological advance, particularly when supplemented by government R&D support and procurement. Coupling between industry and universities played a major role in the development of computing during the 1940s. The first practical electronic machine was built at the University of Pennsylvania. After the war, many of the key scientists and engineers left university and government laboratories to staff fledgling computer manufacturers like Univac.

In the late 1970s, genetic engineering and biotechnology—supported in universities and the laboratories of the National Institutes of Health with Federal dollars—moved rapidly into the private sector, aided by abundant infusions of venture capital. Today, the United States remains well ahead of Japan and Europe in biotechnology. At this stage, coupling among universities, industry, and government makes sense for HTS: much of the research remains well-suited to academic settings; firms in many industries want a window on the technology; Federal agencies are already putting money in.

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[8] See *International Competition in Services* (Washington, DC: Office of Technology Assessment, July 1987), pp. 315-317.

Airline regulation, by limiting price competition, forced carriers to seek other means of differentiating their services. They vied to offer travelers the latest equipment in order to compete on speed and comfort, buying planes from Boeing, Lockheed, and McDonnell-Douglas. These manufacturers, in turn, developed new models.

- *A Federal technology agency.* Strategy 3 considers proposals for altering government responsibilities for science and technology—a subject that goes far beyond the particular needs of HTS. OTA analyzes three variants: a cabinet-level science and technology agency; institutional changes that would substantially raise priorities for engineering research; and direct support for civilian, commercial technologies. The second two hold more promise than the first. Of course, such changes would take time, meaning that they have little to offer in terms of the immediate needs in HTS.

Each of the strategies has advantages and disadvantages. *Strategy 1* is in place and working; the initial Federal response to HTS illustrates the considerable strength of the traditional approach. Mission agencies moved quickly, funneling millions of dollars into HTS R&D in a matter of months. The scientific community moved even faster, with large numbers of skilled professionals shifting into HTS from related fields. The magnitude of the response reflects the sheer size of the pool of scientific expertise in the United States—itself a major source of advantage.

Several weaknesses are apparent in the U.S. response. First, the universities are having a hard time competing for funds; DOE laboratories are getting roughly as much money for HTS research as NSF has for all the Nation's universities. The second weakness: almost complete reliance on mission agencies to support HTS R&D. As a result, not enough R&D money flows to non-defense industries—which might not be a problem, were American firms pursuing HTS as aggressively as Japanese firms. Neither DOE nor the Department of Defense (DoD) can be expected to provide broad support for industrial R&D in HTS. HTS technologies stemming from DoD R&D may eventually find their way into the marketplace, but time lags that made little difference in the 1950s and 1960s, when the United States dominated the technological frontier, can be fatal in the 1980s.

OTA's analysis suggests that **continuing along the lines of Strategy 1—the current approach—will more than likely leave the United States behind** in superconductivity. In mid-1988, the U.S. position in HTS looked like a strong one. But this is because HTS remains largely a matter for scientific inquiry. With progress toward applications, the picture will change. At best, a lead in science creates small advantages, often fleeting. The real contest will be over applications engineering and manufacturing—where Japan excels, and where proprietary technology, much of it developed in industry, will make the difference.

Strategy 2, an aggressive Federal response, would, first of all, assign NSF a greater role in sponsoring R&D. Although basic research in HTS does not seem underfunded, more money for the Foundation—perhaps $20 million over a 5-year period, specifically for HTS—would guard against missed opportunities in relatively fundamental work. With enough funding available for its research center programs, NSF would also be in a position to support one or more proposals for centers dedicated to HTS.

As the second step in a more aggressive strategy, the Federal Government could share in some of the costs of R&D conducted by industry consortia. This is the simplest and quickest way to steer more resources into applied research and generic technology development. Government could direct public funds to R&D industry views with favor, but where the benefits would be difficult for individual firms to capture.

A working group on HTS would be assembled to help carry out this second strategy, and in particular to make decisions on cost-sharing. The aggressive response approach requires agreement on an R&D agenda—a consensus that should not leave out the universities or the national laboratories, even though industry's view of commercial needs would have to come first. The primary task for the working group: deciding which R&D consortia receive Federal dollars—a sticky issue, one involving decisions going beyond the scientific merits of alternative projects.

This strategy skirts the "picking winners" problem raised by Federal subsidies for private R&D by, in effect, allocating government resources to the highest bidders. That is, among proposed R&D consortia—all of which were technically qualified—funds would go to those whose members were willing to make the longest term financial commitment and self-finance the highest fraction of total costs. These criteria would allocate government financing to joint R&D ventures with the greatest expected payoffs over the medium to long term. **The aggressive strategy for commercializing HTS could substantially improve prospects for rapid commercialization of HTS in the United States** at relatively modest cost.

Under *Strategy 3*, OTA addresses prospects for a Federal agency charged with supporting commercial technologies. A perennial issue in U.S. science and technology policy, many alternatives have been proposed over the years. The possibilities range from a small, independent agency with a budget of less than $100 million, to a cabinet-level Department of Science and Technology pulling together some (though hardly all) of the R&D activities of existing agencies.

The cabinet-level Department of Science and Technology came forward once more in 1985 as the lead recommendation of the Young Commission on competitiveness, but found no more support than in the past. Alternatively, is it possible to envision a smaller Federal agency with industrial technology as its mission? Once again, proposals have been common—e.g., for a national technology foundation, paralleling NSF's role in support of science, or a civilian version of DARPA. The latter has attracted particular attention, given DARPA's enviable reputation—a small group of creative people, with the judgment and experience to seek out and support the best ideas. But DARPA has a mission, and a critical one—support of long-term R&D with potentially big payoffs in military systems.

Lack of a comparable mission is the potential Achilles' heel for a civilian technology agency. All such proposals face a common problem: providing money for industrial R&D, in the name of commercialization and competitiveness, *without* a well-understood and widely-accepted mission. (Competitiveness is a notoriously slippery concept—more so than national defense or health.) Lacking such a mission to lend discipline to the process of setting priorities and making funding decisions, a Federal technology agency could easily end up subsidizing marginal projects.

The more of its funds such an agency channeled to industry, the deeper the possible pitfalls. Direct funding of industrial R&D raises the specter of subsidies won by lobbying rather than merit. Yet if the technology is to be useful to industry, then much of it should be developed by industry. Dealing with the many and contentious issues posed by a Federal agency for commercial technology development would be difficult, although not necessarily impossible. If such an agency is to support R&D in the public interest, it will need to find ways of identifying what that public interest is, convincing potential critics that it has done so fairly, and that the results justify continuing support.

Plainly, the three strategies analyzed in this chapter are not exclusive. They do represent differing views of the strengths and weaknesses of the U.S. approach to technology development. Those who believe that the fundamental strengths of the U.S. system remain intact feel that industry will be able to commercialize HTS when the time is right. Those advocating a more aggressive policy stress the dangers of a business-as-usual mentality, given the suprising speed with which U.S. industry has lost its earlier advantages in high technology. The underlying worries over loss of competitive advantage lead those who would favor the third strategy, or something like it, to argue that the United States needs to thoroughly overhaul its approach to technology policy.

STRATEGY 1: FLEXIBLE RESPONSE

The Current Approach

This strategy presumes that the existing policy framework is appropriate and sufficient for supporting HTS.[1] To those who advocate this approach, a major departure from the current course would be premature—at least during the early stages of HTS. With a good deal more basic research required to overcome the technical obstacles posed by the new materials, the

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[1] For general background, see A.H. Teich and J.H. Pace, *Science and Technology in the USA* (Essex, UK: Longman, 1986); also H. Ergas, "Does Technology Policy Matter?" *Technology and Global Industry: Companies in the World Economy*, B.R. Guile and H. Brooks (eds.) (Washington, DC: National Academy Press, 1987), p. 191.

President's initiative (box B, ch. 2), along with other executive branch actions, will provide a sound basis for industry to commercialize HTS —when the time comes. And, so the argument goes, if the pace quickens, or foreign competition intensifies, there will be ample opportunity to agree on a stepped-up response.

This is the *de facto* U.S. strategy. The Federal Government is following traditional channels, relying on existing institutional arrangements, and avoiding direct support for commercial technology development. A continuation of this approach (indeed, almost any approach) will mean:

- Heavy ongoing funding for defense applications of HTS. The Strategic Defense Initiative (SDI) will continue providing a good deal of the money, and DARPA will probably continue to have a prominent place as well. Both industry and universities would get research money—some of the the latter through DoD's University Research Initiative—but within half a dozen years, aerospace firms and military systems houses would probably be conducting the bulk of DoD-sponsored superconductivity R&D. Although DARPA has stated that the results of its processing contracts will remain unclassified, such a policy will be subject to change, depending on outcomes. As R&D moves on to defense-specific applications, classified programs may become common.
- DOE and the National Aeronautics and Space Administration (NASA) would pursue their own mission-oriented projects, with most of the Energy Department's money going to the national laboratories. Much of DOE's support will probably go for R&D and demonstration projects directed at electric power applications. DOE and NASA might pick up a few projects of interest to DoD.
- NSF would continue to fund HTS in the universities, with some of the Foundation's support going to individual investigators and some to research centers.

Other ongoing shifts in U.S. technology policy would proceed along lines suggested in chapter 4:

- The executive branch will continue its efforts to open up the national laboratories, as well as to strengthen university-industry relationships and stimulate technology development and transfer through such initiatives as NSF's Engineering Research Centers (ERCs) and agency Small Business Innovation Research (SBIR) programs.
- The Administration would also continue to press for stronger intellectual property protection, both at home (process patents) and overseas (negotiations with foreign governments aimed at stronger laws and tougher enforcement).
- Some State governments would channel support, direct and indirect, to HTS as part of technology-based economic development programs.
- Venture-financed companies dedicated to HTS would continue to emerge. Private firms, both new and established, would negotiate collaborative R&D arrangements, nationally and perhaps internationally.

Over the next several years, the United States will continue to take the course outlined above. Will such a response, by itself, be adequate? The following analysis indicates that it will not.

Strengths and Weaknesses

In many respects, the current approach to HTS illustrates the great strength of the U.S. system of technology development. Although superconductivity had become something of a scientific backwater by the mid-1970s, NSF for years supported people like Paul Chu at the University of Houston (an institution much like a hundred others below the top ranks in terms of research funding or prestige), and at least a few large U.S. corporations maintained small superconductivity research programs. Moreover, when HTS broke, American scientists could quickly take advantage of facilities ranging from neutron scattering equipment to the National Magnet Laboratory at MIT—facilities already in place, the result of years of Federal funding. Agencies with their own laboratories —DoD, DOE, NASA—began new R&D internally, while contracting out other work.

U.S. scientists not only responded quickly, but in large numbers, as measured by the flood of proposals to NSF, and papers published in professional journals and delivered at scientific meetings. This response reflects the sheer size of the pool of scientific and technical expertise in the United States—a notable strength. It also reflects the flexibility of the U.S. R&D system.

NSF was perhaps the most agile of the Federal agencies, moving quickly to provide funds —largely redirected—to individual research groups and to the Materials Research Laboratories (MRLs). NSF-funded investigators working in related areas were able to shift their attention immediately to HTS, because of the flexibility of Foundation grants.

Scientists with DoD and DOE contracts or grants were also able to move quickly. In addition, DoD redirected millions of dollars in a few months, as various defense agencies exercised their much-valued fiscal autonomy. Each went its own way, with a resulting diversity of technical approaches that is probably healthy overall. Likewise in DOE, laboratories competed to stake their claim in the newly discovered territory of HTS, resulting in an aggresive, if somewhat fragmented, effort. Interagency coordination, though largely informal, has been relatively effective: program managers and contract monitors working in superconductivity know one another and feel a shared sense of responsibility (box M, ch. 4).

Like government, venture capitalists reacted quickly to the new opportunities, lining up technical experts, many of them university faculty members, and quickly investing nearly $20 million in entrepreneurial startups (box G, ch. 3). These startups are just one illustration of close industry-university links in HTS—another significant asset of the U.S. system. The October stock market plunge led at least one HTS startup to cancel plans for a public offering but, overall, availability of capital has not been a major constraint.

In sum, U.S. R&D in HTS will continue to benefit from the unparalleled breadth, depth, flexibility, and diversity of the Nation's research system. It is easy to see why many people feel the current U.S. response to HTS is sufficient—at least for now. But weaknesses have also begun to surface, and others will probably appear over the next year or two.

Funding for basic research in HTS could be a problem, though probably not a serious one. As the ongoing flood of technical papers indicates, the scientific effort remains broad and intense. (At the March 1988 meeting of the American Physical Society, more than 600 of 3,500 papers presented dealt with superconductivity. Most were written by scientists based in the United States.) There are no obvious gaps in fundamental science: people somewhere are pursuing almost every possibility imaginable. More than likely, the ongoing university efforts will suffice to train enough people for industry's eventual manpower needs.

On the other hand, basic research in HTS may already have reached its peak. More sophisticated laboratory equipment will be needed to keep up in the future (e.g., for work on thin films), and costs will rise. Some of the investigators who used existing grants and contracts to move into HTS will have trouble getting new money to continue; they will have to show real promise, rather than routine results, to qualify for ongoing support. And even if there is enough money in total for basic research in HTS, the money might be better spent (box R). As noted earlier, the national laboratories have an HTS budget in 1988 roughly equal to that of NSF, an allocation that seems out of proportion, given the trouble universities have had getting funds.

Even if mission-oriented R&D funded by DOE and DoD were to transfer to the commercial marketplace, it would not do so immediately. The time lags made little difference in the 1950s and 1960s, when American companies were far ahead in technology. Today, the United States cannot afford to wait while know-how diffuses at its own pace from Federal laboratories to the private sector.

Box R.—National Laboratories or Universities?

Postwar U.S. science and technology policy has relied heavily on the university system, with the national laboratories concentrating on mission-oriented R&D. Since the budget cutbacks of the early 1970s, many laboratories have sought to broaden their R&D—a trend that, arguably, has already cut into the share of Federal resources flowing to the universities. With DOE efforts to move beyond the big science role inherited from the Atomic Energy Commission (i.e., big physics) into fields such as mapping the human genome, it may not be much of an exaggeration to say that the Energy Department seems to be trying to become a general-purpose science agency.

This expansion raises issues of balance. Compared to the DOE laboratories, the university system is more open and supports a more diverse set of R&D projects. The universities operate with proven systems of self-governance, intellectual autonomy, and quality control through peer review. Bad science cannot hide for too long. Perhaps most important, industry's need for trained people gives the universities a special claim on Federal R&D funds. No other set of research institutions trains scientists and engineers for industry in large numbers. When these people move to the private sector, they take the latest knowledge with them.

While the laboratories' performance in technology transfer has certainly improved over the five years since the Packard Commission report (ch. 2), some of the remaining problems concern the amount of autonomy that should be given to mission-oriented facilities operated under contract to the Government. In addition, the laboratories are poorly positioned to deal with problems related to manufacturing. Laboratory personnel, unfamiliar with industry and the marketplace, often ignore the need to address processing early enough in development. University engineering departments have also fallen into this trap, but seem to be doing more to dig themselves out. Moreover, universities have been willing and able to work with industry; the DOE laboratories, until recently, have shown few signs of the flexibility needed to adapt their ways to industry's needs. At present, some people in industry view the laboratories with suspicion—and also as competitors for Federal R&D dollars.[1]

The laboratories *can* claim advantages over the university system—including a capacity for interdisciplinary research, sophisticated facilities, and experience with large-scale projects. But, as emphasized in chapter 4, even if policies put in place to change the laboratory system prove successful, the process will take time. Neither the universities nor the national laboratories should or could become centerpieces of a strategy for commercializing HTS. Their strengths lie elsewhere.

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[1] See, for example, "National Labs Struggle With Technology Transfer," *New Technology Week*, Sept. 28, 1987; Allan S. Gelb (Director, Marlow Industries, Inc.), testimony to the House Subcommittee on Energy Research and Development, Committee on Science, Space, and Technology, Oct. 20, 1987.

Granted, the mission agencies are trying to address national concerns over competitiveness in their pursuit of HTS—e.g., through DARPA's processing R&D initiative. However, the near-term focus of the DARPA processing program in HTS may not take advantage of the agency's own strengths—funding of visionary research. Finally, DARPA must ultimately serve DoD missions, which means that when civilian and military needs diverge, commercialization will recede as an objective. Only if the agency can link its HTS R&D with military objectives will it find continued support within the Pentagon. Other problems aside, the program is relatively small—only about $18 million in 1988—and it was not fully underway as this report went to press.

In short, the lack of more direct mechanisms for Federal support of commercially oriented R&D has become a weakness in the U.S. approach to technology development. The issue is not overall funding levels for HTS. The issue is the allocation of those funds: mission-oriented R&D does not provide enough support for commercialization to ensure that American firms will be able to keep up in HTS.

The problem is particularly acute because of the wait-and-see attitude in much of American industry. As described in chapter 3, the Japanese are putting more effort into exploring applications. Furthermore, Japanese companies, with their strengths in engineering and manufacturing, would probably be able to catch up even if U.S. firms were first to reach the market with innovative products.

In sum, the current U.S. response to HTS displays the strengths and weaknesses that have characterized the performance of American companies in high technology. The U.S. effort looks formidable in the middle of 1988, but that is to be expected: the challenges so far have been largely matters for the research laboratory. If there is a surprise, it is that the Japanese—not known for innovation in science—have already posted such a strong showing.

As HTS moves toward applications, science will recede in importance. Basic research results, by their nature, will diffuse rapidly, providing little in the way of national advantage in commercialization. (Patent coverage sufficiently broad and strong to lock up a critical class of HTS materials seems unlikely.) Rather, the critical technological advantages are likely to reside in proprietary know-how associated with processing and fabrication techniques and design-manufacturing relationships—precisely where Japanese companies have demonstrated an advantage over many of their American counterparts.

A continued response along the lines of Strategy 1 is quite likely to fall short: **the widely expressed fear that Americans will win in science, while the Japanese take the commercial markets could come true.** Support for science and for military technology—the essence of this strategy—served the United States well from 1950 to the middle 1970s. But the lesson of the past 15 years is clear: in a world of increasingly effective national competitors, these two levers no longer suffice.

STRATEGY 2: AN AGGRESSIVE RESPONSE TO HTS

Three primary features distinguish this second strategy from the current approach:

1. A larger role for NSF, both in funding individual research at universities, and through the establishment of one or more university centers in superconductivity.
2. Federal cost-sharing of long-term, high-risk R&D planned and conducted by industry consortia.
3. A working group on commercialization of HTS charged with helping shape consensus on an R&D agenda, and making decisions on Federal cost-sharing.

This strategy preserves the strengths of the traditional U.S. approach to technology development, while compensating for the weaknesses brought out by stronger international competition—i.e., lack of breadth in industrial R&D, and heavy reliance on mission agencies for Federal support.

Step One: A Larger Role for NSF

The initial step toward a more aggressive response to HTS should be straightforward: give the National Science Foundation more money for university research. For reasons outlined earlier in this report, a dollar spent by NSF should contribute more to commercial development, on the average, than a dollar spent by DoD or DOE. In view of this, NSF's existing 15 percent share of the total Federal R&D budget for HTS is too small.

There are two complementary ways for Congress to expand NSF's role. An otherwise unrestricted appropriation, earmarked for HTS—money that the Foundation could spend on superconductivity as it sees fit—would permit NSF to fund some of the highly rated HTS proposals that it is currently forced to turn away.

As noted above, OTA has found no evidence of serious underfunding in basic research on superconductivity. Nonetheless, with an NSF research budget that has been flat in real terms for several years, funds for condensed matter physics were cut back during fiscal 1988.[2] (Ch. 4, which discussed this and other symptoms of the pressure on the NSF budget at some length, included a number of policy options addressing the general problem.) As part of Strategy 2, **Congress might consider appropriating, say, $20 million (in additional new money) for NSF for the 5-year period beginning in fiscal 1989, specifying that the funds go for HTS research.** Such a step would help ensure that the basic science underlying superconductivity gets adequate support, without cutting into budgets for NSF-sponsored R&D in other fields.

In addition (or as an alternative), Congress could **authorize and appropriate funds to NSF specifically for one or more university centers dedicated to HTS research.** To give NSF maximum flexibility, the centers could be established under one of several existing programs, or through a new program altogether.

While none of the Foundation's existing or proposed center programs (discussed in ch. 4—see especially box N) seems ideally suited to the needs in HTS, an ERC comes closest. Although a number of the MRLs have good experimental facilities, and active research in superconductivity, industry involvement has rarely been a major goal. Nor are the proposed S&T centers, although emphasizing multidisciplinary research, likely to focus as strongly on industry interactions as the ERCs. While the program is relatively new, and as yet few of the ERCs have themselves demonstrated close working relationships with the private sector, their focus means the ERC program fits the needs in HTS more closely than other candidates. (None of the existing ERCs has a research agenda embracing HTS.)

Several bills introduced in the 100th Congress—e.g., H.R. 3048 and H.R. 3217—would instruct NSF to establish a program for interdisciplinary National Superconductivity Research Centers. Would a new center program for HTS—one with the explicit mission of building a strong technology base for commercialization, and one with teeth in the requirements for multidisciplinary work and industry involvement—do more for HTS than funding for one or more ERCs or S&T centers? The answer has to be yes, if Congress appropriates the money and if NSF moves relatively quickly. (The Foundation would have to solicit new proposals, while it already has proposals in hand for S&T centers on superconductivity.)

In sum, congressional funding for several (say, one to three) new multidisciplinary centers in HTS could represent a modest but important step. It would not be realistic to expect such a measure to expedite commercialization dramatically. University centers, even at major schools, would no doubt remain relatively small in scale and scope. Sums of $10 million to $20 million annually (perhaps $5 million, at most, per center) are about the maximum that make sense for an NSF center program—the Foundation does not do business on a scale much above this. Such centers would serve primarily as a source of new ideas and trained people—an important contribution to commercialization, not to be undervalued.

Step Two: Industrial Consortia with Federal Cost-Sharing

As another element in a more aggressive strategy, **Congress could direct the Administration to partially offset the costs of joint industrial investment in long-term, high-risk R&D.** Such a policy—designed to address the gaps in HTS R&D in U.S. industry—would be based on two premises discussed at length in this report. First, just as the Federal Government supports basic research, it must bear part of the burden

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[2] [illegible] by the Division of Materials Research during 1987 [illegible] the expectation of a substantial budget increase [illegible]. Not only NSF, but DoD and DOE, maintain that a considerable number of highly rated research proposals are going unfunded. The problem is not a new one, particularly for NSF. But the problem has gotten worse, and program managers understandably feel uncomfortable trying to draw lines between proposals that are almost indistinguishable in quality.

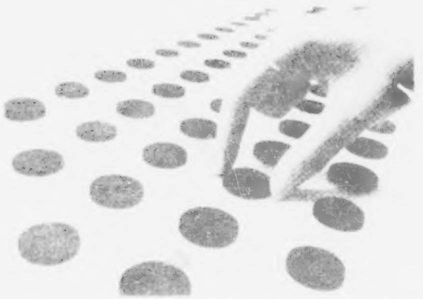

Photo credit: Superconductive Components, Inc.

Pellets of HTS ceramic material

of exploring risky, radical industrial technologies, which often provide large public benefits but only small private returns. Second, although DoD has borne much of this burden in the past, the growing specialization of defense technologies, and continuing pressure to meet the immediate needs of the services, mean that the United States now lacks a consistent champion for major new technologies with potential impacts on the civilian side of the economy.

Partial Federal support for one or more industry consortia—established, as discussed in box S, to share R&D costs—is a simple and workable policy to address this problem. Financing some fraction of joint industry efforts with Federal funds (or tax expenditures) would pull more resources into applied research and generic technology development, and raise the overall level of R&D investment. The approach would direct public funds into areas that industry itself thinks will have the highest payoffs, but where the benefits would be difficult for individual firms to capture.

In some respects, the approach envisioned for HTS resembles that of Sematech, the microelectronics industry's new R&D consortium (to which more than a dozen firms have pledged 1 percent of their revenues). While providing substantial funding, the Federal Government would be a largely silent partner, with an R&D agenda put together and managed by member firms. And as with Sematech, there would be explicit linkages with universities and DOE national laboratories, so as to tie publicly-funded basic research to the joint R&D.

The differences between Sematech and the HTS R&D consortia envisioned for Strategy 2 are perhaps more important. First, the semiconductor industry itself proposed and fought for a Federally-supported venture. The companies likely to be involved in HTS consortia have made no such effort, and are not likely to; thus, the job of initiating and organizing such programs would fall in part on Government.

Second, DoD, through DARPA, serves as the financial channel to Sematech. A report by a Defense Science Board Task Force argued that the industry's troubles imperiled national security—one reason for DoD's oversight role. Moreover, DoD was perhaps alone among Federal agencies in having the technical expertise to monitor microelectronics R&D. Even if the DoD arrangement proves satisfactory in the case of Sematech, it holds small promise as a model for HTS. DoD—as well as DOE and NSF—could be involved with HTS consortia, but none of these agencies should oversee them, lest their purposes be subordinated to ongoing agency missions. This point is discussed further in the following section, which deals with institutional mechanisms.

Finally, Sematech has a focused R&D agenda, stemming from a consensus within the industry that manufacturing technology has been a major source of competitive difficulty. Rather than a single, well-defined focus, HTS lends itself to multiple agendas, different sets of participants. Three among many possible candidates:

- *Electric Utility Applications.*—Utilities normally make highly conservative investment decisions. They are unlikely to adopt a new technology until sure it will work reliably for many years. Thus, there may be a useful role for Government in accelerating the development of the engineering database and field service experience in HTS through support for cooperative projects (which could involve DOE laboratories).

Box S.—Collaborative R&D[1]

The breakthroughs in superconductivity brought forth many proposals echoing the theme of strength through collaboration. The President's initiative urged Federal agencies to cooperate with universities and the private sector. Legislation has been introduced in Congress with similar objectives. In part, these calls for collaboration represent a response to rising R&D costs and the loss of U.S. technological advantage. To some extent, they stem from a misapprehension of the sources of Japan's success (the myth of cooperation discussed in ch. 3).

Although joint research is nothing new, the past decade has seen a steady growth in U.S. R&D consortia, and a marked change in research focus. Firms that once cooperated only on matters such as technical standards have increasingly banded together to undertake pre-competitive R&D—projects on new technologies with direct commercial relevance. The best-known—the joint venture Microelectronics and Computer Technology Corp. (MCC)—has begun exploratory work on HTS.

The economics of joint R&D hold considerable appeal. By pooling resources and avoiding duplication, cooperative research increases the potential leverage of each firm's R&D budget, while limiting the costs to any one firm of a failed project. Joint efforts also enable participants to monitor developments in a technical field without developing a full capability in-house.

While the benefits of cooperative research are sizable, so are the limitations. Most important, joint endeavors cannot substitute for in-house R&D efforts, only complement them. The participating firms must absorb the results, and transform them into commercially relevant products or processes—something that requires a sophisticated and independent internal effort. For such reasons, government efforts to encourage cooperative research programs in industries where firms pursue little or no R&D on their own have seldom succeeded.[2]

A second limitation may prove more serious. Ideally, cooperation in research should help lengthen project time horizons; but many of the participants in cooperative ventures shift R&D strategies in two ways: 1) by focusing their internal research on still shorter-term work; and 2) by seeking to move the cooperative's agenda away from basic research and toward more applied projects. The trend has been evident in MCC, which recently restructured its largest program—in advanced computer architectures—to emphasize more immediate paybacks.

Collaborative efforts involving universities (or government laboratories) have an easier time focusing on long-term research, often with little interference from participating firms, because the firms are not seeking specific R&D results so much as access to skilled graduates and faculty expertise. Of course, the financial commitments are generally much smaller than those for participation in a joint venture such as MCC (and may be viewed in part by the firm as good corporate citizenship). While cooperative R&D programs housed in universities help maintain a strong technological infrastructure, they should not be viewed as engines of commercialization.

Finally, the sheer difficulty of organizing and managing a collaborative research venture creates its own set of limits. Fundamental issues—reaching agreement on an R&D agenda, finding ways to share technologies and business information, controlling costs and determining intellectual property rights—can pose enormous obstacles, particularly when the collaborators are also competitors. These are among the reasons that cooperative research accounted for only $1.6 billion of the more than $50 billion spent by American industry on R&D in 1985. Of this total, 85 percent ($1.4 billion) went to support R&D cooperatives in the communications, gas, and electric utility industries, whose members do not compete directly. It is no surprise to find that industry leaders rarely put cooperative R&D very high on the list of steps needed for rebuilding U.S. competitiveness.[3]

Joint R&D has a role to play—in HTS and in solving the more general problems visible in the U.S. technology base—but that role will inevitably be circumscribed by tensions between competition and cooperation among the participating companies. Firms seek proprietary technologies in order to compete with one another. Cooperation between firms in the same business can only go so far; cooperation between firms having supplier-customer relationships, or those in different industries with common R&D goals, holds more promise.

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[1] See "Collaborative Research: An Assessment of Its Potential Role in the Development of High Temperature Superconductivity," prepared for OTA by D.C. Mowery under contract No. H36730, January 1986.

[2] NSF's Industry/University Cooperative Research Center program (IUCR, box O, ch. 4) was initially designed to substitute for in-house R&D in technologically moribund industries. An NSF evaluation found that "Companies with little research background, such as the utilities and furniture companies, are traditionally conservative with respect to new technology and depend on their suppliers for whatever changes they adopt." *An Analysis of the National Science Foundation's University-Industry Cooperative Research Centers Experiment* (Washington, DC: National Science Foundation, 1979). IUCR programs h[illegible] gone on to more success in other industries.

[3] For the results of a recent survey of corporate executives, see "The Role of Science and Technology in Economic Competitiveness," final report prepared by the National Governors' Association and The Conference Board for the National Science Foundation, September 1987, pp. 20-30. The R&D spending figures for cooperatives come from P.F. Smidt, "US Industrial Cooperation in R&D," remarks at the Annual ESPRIT Conference, Brussels, Belgium, Sept. 25, 1985.

- *HTS Magnets.*—Strong magnetic fields, by dampening thermal fluctuations, can give purer, more uniform crystals of silicon and other semiconductor materials, making this a natural candidate for superconducting magnets. R&D directed at HTS-based magnets could, at the same time, help move HTS out of the research laboratory and lessen U.S. dependence on foreign sources of semiconductor wafers. X-ray lithography using compact synchrotrons—a promising candidate for making next-generation integrated circuits—likewise could serve as a spur for HTS R&D while filling a chink in U.S. microelectronics capabilities.
- *Superconducting Computer Components.*—HTS interconnects may help improve the performance of computers. Hybrid semiconducting/superconducting electronics may also prove viable. Collaborative projects on prototype circuitry, with subsequent Federal purchases of very high-performance machines if the technology panned out, might help speed commercial development of HTS, while at the same time preserving the U.S. lead in high-end machines. Such a program would have a substantial basic research component, but involve manufacturability and applications issues as the technology began to mature.

Whatever the substantive agenda, the private sector should take the lead, insofar as practical, with Government participation limited to that necessary to achieve the Government objectives—leveraging critical R&D, filling gaps in the technology base, lengthening time horizons. Federal cost-sharing justifies such procedural rules as these:

- *Government participation should be conditional on significant investment by industry*—say 50 percent or more. (The Sematech formula—40 percent from both industry and DoD, and 20 percent from State and local Government—provides an alternative.) The less of their own money companies contribute, the greater the risk of R&D that strays from marketplace needs. So long as Government funding decisions can be limited to choosing among alternative approaches, all of which industry is prepared to back, some of the problems of picking winners can be skirted.
- By also requiring, as a minimum, *a 3-year financial commitment on the part of consortium members*, the Government will have added assurance that public funds will help support medium- to long-term R&D. (MCC requires only a 1-year commitment, down from 3 years initially; this change is both cause and consequence of pressures for tangible early R&D results.)
- *Companies should be either in or out.* MCC, which permits members to join projects selectively, has found itself trying to wall off some of its work to prevent R&D results from leaking to companies that have not joined a particular project.
- *Entrance requirements should be transparent*, lest the consortium become a smokescreen for anti-competitive behavior; any eligible U.S. firm should be able to join (those without majority U.S. ownership might reasonably be barred).
- People transfer technologies most effectively. To see that knowledge flows out of the consortium, *employees of the member companies should be heavily represented among the R&D staff.* It seems reasonable to insist that half or more of a consortium's staff come from member companies (rather than new hiring). Assignments might be temporary, but should be long enough—perhaps 6 months as a minimum—for meaningful contributions to R&D, and for learning purposes.

  Furthermore, member firms should be encouraged to send their best people. MCC dealt with this by retaining—and exercising liberally—the right to reject employees sent by shareholders (initially, it turned down 95 percent).[3] University involvement can also help attract the best industry scientists.
- No less important if the consortium is to affect commercialization, *member firms must conduct ongoing complementary R&D*

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[3] B.R. Inman, "Collaborative Research and Development," *Commercializing SDI Technologies*, S. Nozette and R.L. Kuhn (eds.) (New York: Praeger, 1987), p. 65.

of their own. MCC encourages "shadow research" by members, paralleling the joint venture's work. This ups the ante for members; Digital Equipment Corp. spends half again its investment in MCC seeking ways to use the consortium's results—a level of commitment that has, however, been rare. Parallel efforts will be particularly important for firms with little or no experience in superconductivity. They will have more learning to do than companies with backgrounds in, say, LTS.

- If one purpose of Federal funding is to stimulate visionary R&D, *it may make sense to discourage too much publicity*. By reducing the pressures—political and other—for short-run success stories, the chance of success stories over the longer term should go up.

  A long-term orientation also suggests that a consortium's work stop well short of full-scale commercial development efforts—i.e., at the prototype stage, leaving further development to the members' own efforts.

Step 3: A Working Group on Commercialization

The final step in the aggressive strategy would be to establish a working group of experts—drawn from industry, universities, and government—with a limited mandate to promote the new technology. Such a group—with a lifetime fixed at, say, 10 years—could serve a number of important functions.

The first is *fact-finding and analysis*. Currently, no public or private body has a continuing responsibility to provide authoritative policy guidance concerning such questions as: What problems do we need to start on now to assure rapid commercialization? How much money will it take? Are some HTS R&D areas getting too much money? Which areas are not getting enough? While such questions never have definitive answers, the first step toward good decisions—given that the Federal Government will have to make decisions in any case—is to understand what is going on in both government and industry, here and in other countries. The problem is not inadequate coordination. Rather, the problem is that no one has the task of drawing even a crude map of the road to commercialization, and setting the necessary priorities along the way. To do so will require solid and timely analysis, on a continuing basis. (The President's Wise Men's advisory committee on HTS will evidently prepare a one-time report, rather than provide ongoing policy guidance.)

While the working group's responsibilities would involve decisions on Federal cost-sharing in response to proposals from private sector consortia, it might first have to engage in *consortium-building and facilitation*. In contrast to Sematech, HTS consortia are not likely to organize themselves spontaneously, at least until guidelines for Federal cost-sharing have been set down. The working group might be able to play a match-making role, helping bring together companies, universities, and government laboratories, and aiding them in reaching consensus on research needs—a function that could continue even after the joint R&D effort was underway; as the experience of Sematech and many other joint ventures demonstrates, conflict will be inherent in any consortium of independent firms.

The working group's ability to get things done will flow in part from its power to *allocate Federal resources*. Decisions on who is to get Government money will hinge not only on technical questions, but on economic judgments. Technical evaluations of competing projects can rely on the tried and tested approach of external review by recognized experts without a stake in the outcome.

Evaluating the economic merits—i.e., likely impacts on commercialization—would be more difficult, but the problem can be sidestepped to considerable extent by using procedural rules to allocate public funds. First, a consortium's proposed R&D agenda would have to meet minimum criteria, which the working group would set, based not only on technical merit, but, as discussed above, on rules for participation, and provisions for member funding and R&D time horizons. Then, among the qualifying proposals, funds would go to the "highest bidders" as measured by length of time commitment participants in the consortium were willing to make, and the fraction of total costs they were willing to self-finance. (A third criterion—the investments of members in complementary internal R&D—is also relevant, but it would be difficult to determine what was "complementary" and what was not.) These measures should help to allocate government resources to joint R&D projects with the longest term payoffs, and the greatest value to industry.

If the working group makes decisions on funding, the issue of administering such a program remains. As an *ad hoc* body outside the ordinary apparatus of Government, the working group would need to look to an existing agency for help with staffing and managing its responsibilities. This poses a dilemma. As discussed above, the working group is not simply an advisory body, but a center for decision-making on commercialization policies. Yet only a minority of its members would be Federal employees, it would go out of existence after some period of years, and the intent is not only to complement the activities of existing agencies but, to considerable extent, to substitute for the agencies—to undertake tasks that they do not (and perhaps cannot). Attaching the working group, even for administrative convenience, to an existing agency could undermine its impact.

Each of the three agencies heavily involved in funding HTS R&D—DoD, DOE, and NSF—has noteworthy strengths: experience in funding LTS research, technical competence, and the administrative tools needed for monitoring the expenditure of Government funds. But each has flaws as well: DoD's military mission will always come first; later if not sooner, commercial technology development will probably devolve into a secondary objective of consortia with Pentagon involvement. DOE has less experience with the private sector than DoD—e.g., in managing extramural R&D—and a narrower base of technical expertise. For NSF, the assignment would be a substantial departure from the norm; the Foundation has limited ties to the private sector, and few employees with industrial experience. Its past attempts to foster applied research have not met with great success.

Are there other possibilities? The Commerce Department is seldom seen as a technology agency. The Office of Science and Technology Policy (OSTP), as pointed out in the preceding chapter, has a small staff and is not set up to handle the kind of tasks the working group would need to pass along. (A Federal technology agency, as described in the final section of this chapter, might be well-suited; but even if Congress were to pass legislation creating such an agency, it would not be ready in time to serve the working group.)

In the end, the best solution is probably to set up the working group as an *ad hoc* independent body, with a small staff of its own, and attach it to OSTP. As such, it should have the necessary qualities for promoting commercialization of HTS: the flexibility and substantive depth to learn by doing, tailoring its procedures to the special needs of HTS as these became apparent.

The mission agencies will take care of their own needs in superconductivity. What is lacking is an organization to look after the broader national interest in commercializing this new technology. Federal support of joint private-sector R&D investments addresses the need. This is not the only alternative—box T summarizes others—but it seems a promising one.

STRATEGY 3: A FEDERAL TECHNOLOGY AGENCY—THREE ALTERNATIVES

The Federal structure for science and technology policy has changed little since the late 1950s. Within DoD, the Office of Naval Research set the post-1945 pattern for support of R&D. In 1950, after prolonged debate, Congress passed the authorizing act for the National Science Foundation. The same year saw major new legislation setting the National Institutes

Box T.—Aggressive Support for Commercialization: Other Possibilities

Flagship Projects

The symbolic and potential economic importance of HTS have led to calls for a flagship approach. With HTS already a symbol, among other things, of U.S.-Japan economic competition, advocates of the flagship alternative see virtue in Government initiatives that likewise have symbolic value. A flagship should rally industry, universities, and the public sector, building on the enthusiasm created in the media and galvanizing the Nation's creative and entrepreneurial vigor in a race, not to the moon, but over the hurdles of some earthbound Olympics--an Olympics of science, technology, and competitiveness. This country's strength is in meeting crises—the Manhattan Project, Sputnik—not in incrementalism, say advocates of this approach. Political consensus, and a bold national effort, could pull superconductivity out of the laboratory and into the global marketplace. Visibility can be a strength as well as a potential source of weakness.

As appealing as such images might seem, HTS is not the kind of technology that lends itself to a massive, concentrated effort. Although bills have been introduced dealing with magnetically levitated trains, public and political attitudes toward rail transportation would probably have to change a good deal before maglev would have broad appeal. Talk of an energy crisis evokes little response today. Superconducting computers will just be black boxes; no one much cares what is inside. Defense systems do not fill the bill either.

There is another dilemma. A flagship has visibility. It must succeed, if only for such reasons. This forces technological conservatism on decisionmakers. Apollo's achievements were in systems engineering and large-scale project management, not in revolutionary technologies; space is no place for trying out unproven technologies. Pressures for success—or pressures to avoid the appearance of failure—mean safe choices by the managers of such projects. If the goal is Government support for long-term, risky technology development, the flagship approach has little to offer.

DoD Processing R&D

Processing will be vital in commercializing HTS, and several legislative proposals have made it a central element. For example, H.R. 3024 would authorize $400 million for a 5-year, DARPA-centered effort also involving DOE, NSF, and NBS.

DARPA has left a deep imprint on U.S. high technology, most of all in computers. Chapter 4 discussed DARPA's current HTS processing initiative—an effort that could fill an important gap in superconductivity R&D. Giving DARPA the lead in a more ambitious effort, as in H.R. 3024, might seem appropriate. On the other hand, programs aimed explicitly at (non-defense) commercialization fall well outside DARPA's historical mission and experience; in part because it seems to critics in the Pentagon too far removed from military needs, DARPA's HTS program has not found widespread support inside DoD. Congressional enthusiasm—reflected in a direct appropriation—made the program possible, but if [illegible] loses interest, the program could fade away. Only if DARPA could link the R&D with military objectives would it get internal support in the face of budget pressures and competing DoD demands.

In the past, such pressures have periodically led DARPA to abandon longer-term R&D and embrace more immediate military objectives. DARPA's transfer of the MRLs to NSF marked the first of these periods (box O, ch. 4). After the end of the Vietnam War, fatter R&D budgets enabled DARPA to move back toward long-term research. But in the early 1980s, the pendulum swung once again, with the Strategic Computing Program a prime case in point; DARPA has channeled much of the program's funds to military contractors, rather than the university laboratories responsible for most of its earlier successes in computing technology, and set program objectives that will appeal to the services. Ironically, over the last few years, DARPA has behaved much like American corporations—stressing projects with quick payoffs. Finally, there seems little question that major breakthroughs in HTS resulting from DoD-sponsored R&D would be classified, should the Pentagon feel that, as a practical matter, they could thereby be kept from the Soviet Union.

The DOE Laboratories

Although no one has formally proposed the designation of a lead laboratory for HTS, the suggestion has been in the air. There are at least 10 DOE laboratories with work of one sort or another underway in HTS.

The chief argument for greater concentration and centralization is one of efficiency: with all the laboratories on the HTS bandwagon, duplication of effort will be hard to avoid. By giving a clear mandate to one, DOE should be better able to manage the division of labor. On the other hand, with HTS remaining primarily a matter of research—research with fuzzy objectives—centralization for its own sake has little to offer. The conventional management wisdom that basic research is cheap, the benefits of competition among scientists great, makes central control unnecessary and undesirable. Nor is there an obvious candidate for a lead laboratory in HTS.

of Health on a course it has continued to follow.[4] At the end of the 1950s, the Soviet Sputniks spurred another set of changes: the establishment of NASA and DARPA. NASA grew out of the National Advisory Committee for Aeronautics (NACA). Founded in 1915 to conduct research and testing, NACA remained small until World War II, when its staff grew to nearly 7,000 people. NASA's staff eventually reached five times that level, while its budget grew even faster (NASA contracted out much more of its work). DARPA, set up in 1958—and originally given the mission of developing a U.S. space program, later passed to NASA—quickly established itself as the home of long-range R&D within the Defense Department.

Since this period, the Federal R&D budget has grown steadily, but organizational changes have been minor. In 1950, the Federal Government spent about $1 billion on R&D. Today, half a dozen Federal agencies each spend over $1 billion annually, and more than a dozen others spend lesser amounts. Given this growth in R&D spending, and the increasing concern over the Nation's ability to utilize its technology effectively, many proposals to reorganize Federal science and technology functions have come before Congress during the 1980s. (This is nothing new: in 1913, during the debate preceding the formation of NACA, some of the opponents of a new organization for aeronautics research saw it as a stalking horse for a cabinet-level science department.)

An Umbrella Agency for Science and Technology

The more ambitious sounding proposals often call for a science and technology (S&T) agency to consolidate Federal R&D functions. Advocates of consolidation argue that an umbrella organization would lead to clearer priorities, less duplication, and greater efficiency—in a word, to better management. They point, for instance, to the more than 700 national laboratories, managed, often quite loosely, by many different agencies, and note the frequent criticism that the laboratory system has come under.

In fact, calls for a Department of Science and Technology tend to be a bit misleading. Because the mission agencies control most of the Federal R&D budget, the resulting changes would necessarily be modest. When the Young Commission called for a cabinet-level S&T agency in 1985, it offered no guidance on how such a proposal might be implemented.[5] The problem is clear enough. Some 70 percent of Federal R&D goes for defense and space. Much of the rest pays for health-related research.

It is hard to envision moving more than a few bits and pieces of DoD's current R&D—say a billion dollars or so—into another part of Government. Moreover, since creating the Atomic Energy Commission in 1946, Congress has kept nuclear weapons research isolated; currently, nuclear weapons account for about half of DOE's R&D budget.

The second largest R&D agency, Health and Human Services (DOE is third), operates a research arm—NIH—with a hundred-year tradition of excellence. Why risk disrupting organizations like NIH in the name of management efficiency?

Even the strongest advocates of an S&T department acknowledge that consolidation could not go too far without harming the ability of agencies to manage R&D in support of their own missions. But without pulling much of the R&D that is currently the responsibility of these agencies under the new S&T umbrella, there would be little left. It would be hard to take seriously a cabinet-level S&T agency that would oversee perhaps 10 percent of the Federal R&D budget.

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[4] See, for example, J.A. Shannon, "The National Institutes of Health: Some Critical Years, 1955-1957," *Science*, Aug. 21, 1987, p. 865.

On NACA and NASA, below, see F.W. Anderson, Jr., *Orders of Magnitude: A History of NACA and NASA, 1915-1980*, NASA SP-4403 (Washington, DC: National Aeronautics and Space Administration, 1981); and A. Roland, *Model Research: The National Advisory Committee for Aeronautics, 1915-1958*, vol. 1, NASA SP-4103 (Washington, DC: National Aeronautics and Space Administration, 1985); also "Collaborative Research: An Assessment of Its Potential Role in the Development of High Temperature Superconductivity," prepared for OTA by D.C. Mowery under contract No. H36730, January 1988, pp. 29-34.

[5] *Global Competition: The New Reality*, vol. I (Washington, DC: U.S. Government Printing Office, January 1985), p. 51. The Commission simply said that the department should include "major civilian research and development agencies."

For extensive discussion of proposals for an S&T agency, see the special issue of *Technology In Society*, vol. 8, Nos. 1/2, 1986, entitled "A Department of Science and Technology: In the National Interest?"

Given this dominance of R&D by the mission agencies, most of the legislative proposals for reorganization have had quite modest objectives. H.R. 2164, for example, is fairly typical. This bill—introduced in the 100th Congress to create a Department of Science and Technology—would pull together NSF and the National Bureau of Standards (NBS), together with several smaller Commerce Department programs, while also also creating a National Bureau of Technology Transfer and an Advanced Research Projects Foundation. The latter—charged with supporting generic, industrial R&D—represents one variant of a recurring proposal—a proposal that, according to the analysis below, has more in its favor than an umbrella agency for science and technology.

Higher Priorities for Technology and Engineering

Proposals for a technology agency that would stand alongside NSF—perhaps called a National Technology Foundation (NTF)—start with the premise that technology does not always depend on science. Indeed, development of new technology—a goal-directed, problem-solving activity—differs fundamentally from scientific research. Even where the interrelationships are close, as they often are in high technologies, the two activities depend on different kinds of people, with different skills and expertise. Science seeks understanding. Technology seeks satisfactory solutions to practical problems. Science looks to technology for tools—computers to unravel the structure of DNA, or to guide powerful telescopes as they scan the heavens. By the same token, technology looks to science for tools: knowledge of DNA leads to new pharmaceutical products; theoretical insights into computer software now guide the design of hardware.

U.S. problems in commercialization lie in technology, not science. And while scientists make their contributions to innovation and competitiveness, the engineering profession carries much of the burden (ch. 2). **Raising priorities within Government for engineering research—work directed at technology rather than science—would be a straightforward and positive step toward renewed competitiveness.**

Photo credit: Biomagnetic Technologies, Inc.

Brain scan using equipment incorporating superconducting quantum interference devices (SQUIDs)

NSF has made considerable progress at this in recent years, most notably through its ERC program, while amendments to the NSF charter have also given engineering more prominence. Moreover, the Foundation's current director, Erich Bloch, who came to NSF from industry, has provided strong leadership for initiatives such as the ERCs. But Bloch will not be there forever. And NSF's fundamental job is the support of science—science for its own sake. The Foundation cannot tilt too far toward engineering without provoking a strong reaction from its primary, and well-organized constituency—university scientists. Indeed, the ERCs have already provoked such a reaction. NSF is unlikely to shift its priorities much further unless pushed from the outside.

Currently, about 10 percent of NSF's budget goes for engineering. It is hard to envision the Foundation, as presently constituted, increasing the proportion for engineering to more than 15 or 20 percent. The Engineering Directorate's budget has been spread among several thousand departments in the Nation's nearly 300 engineering schools. With relatively low levels of support from NSF, faculty have turned to DoD for money, skewing research toward specialized military problems. (This trend affects curricula and course contents as well, although less directly.)

An NTF, independent of NSF and DoD, could be a powerful lever for moving university research back toward the civilian side of the economy, and for steering engineering education back towards practical industrial problems. Nonetheless, OTA's past analyses have found restructuring NSF—making it, say, into a National Science and Technology Foundation—to be a more attractive option than creating a separate National Technology Foundation.[8] Science and engineering *do* depend on one another. Thus, **an integrated agency, charged with supporting *both* engineering and science, makes more sense than two parallel agencies—provided sufficient resources can be guaranteed for engineering**.

In this variant of Strategy 3, with the focus on engineering research in the universities, impacts on commercialization would be long-term and indirect—both a strength and a weakness. Government money would not go directly for commercial technology development in industry, avoiding the problems such a step would raise. But it would take time before the resources flowing to new research in engineering could make a difference for competitiveness and commercialization. In particular, creating an NTF would not do much for HTS.

Other suggested reorganizations would target industrial technologies more directly. Box U, for example, discusses one recent proposal, in this case a reorientation of NBS.

An Agency for Commercial Technology Development

For years, DARPA has enjoyed an enviable reputation: an elite band of non-bureaucrats, able to pick technological winners and drive them forward. Why not, many have asked, do the same on the civilian side of the economy? The response follows just as quickly. DARPA can pick winning technologies because it has a reasonably clearcut mission, whereas a civilian DARPA would have a much fuzzier charge. Nonetheless, at a time when U.S. industry has lost ground competitively, the notion of a civilian DARPA holds considerable appeal—an agency devoted to championing high-risk, long-term projects, technologies that could make a real difference in international competition. The huge U.S. trade deficit, and especially the imbalance with Japan, is today's Sputnik. Somebody has to do something.

DARPA has, in fact, been able to anticipate technologies important to civilian industry. Although commercialization *per se* has not been DARPA's goal, for most of the agency's history the defense mission has not tightly constrained its decisions. Rather, DARPA has invested in what it regarded as high-payoff technologies, on the rationale that DoD would ultimately benefit as a purchaser.

If DARPA can make make technically sound decisions, a civilian agency should also be able to do so. But the DARPA analogy can be taken only so far. A civilian DARPA, by its nature, would be much more difficult to run efficiently: nurturing new technologies intended to succeed in the marketplace is a more complex and exacting undertaking than supporting a technology for which the Government is the end user. In addition, a civilian DARPA would have high visibility politically. Technology development is now seen as the *sine qua non* of economic prosperity. This means that a civilian DARPA would be under strong pressure from

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[8] "Development and Diffusion of Commercial Technologies: Should the Federal Government Redefine Its Role?" staff memorandum, Office of Technology Assessment, Washington, DC, March 1984.

Box U.—An Advanced Technology Program

Among reorganization proposals, the proposed Technology Competitiveness Act has come closest to implementation. The bill—incorporated in the omnibus trade package passed by Congress in the spring of 1988 (and vetoed by the President)—would:

- rename the Commerce Department's National Bureau of Standards (NBS) the National Institute of Standards and Technology;
- authorize regional centers for the transfer of manufacturing technology;
- provide for technical assistance to State technology programs;
- establish a clearinghouse on State and local initiatives on productivity, technology, and innovation;
- create an Advanced Technology Program as part of the revamped NBS.

The technology transfer and State assistance provisions in the bill could be useful. But it is the Advanced Technology Program (ATP) that would most directly address U.S. needs for industrial technology.

The ATP would assist businesses in applying generic technologies, and in research needed for refining manufacturing technologies and for rapid commercialization of new scientific discoveries. This would be accomplished through, among other things, aid to joint R&D ventures (including ATP participation in such ventures under some circumstances). The bill also authorizes cooperative agreements and contracts with small business, and involvement of the Federal laboratories in the program. Although the trade bill itself does not appear to specifically authorize appropriations for the ATP, a predecessor bill in the Senate (S. 907) would have authorized $15 million for the ATP in its first year.

special interests, States, and Congress itself to steer resources to particular projects. In other words, a civilian DARPA could easily become a pork barrel. Political interests could override economic sense.

Could a Civilian Technology Agency (CTA) avoid these pitfalls? What might it look like, and what it would do? In many versions it would be small and lean (DARPA's staff numbers about 125), emphasizing flexibility and making use of experienced professionals on temporary assignment from the established mission agencies. In more expansive alternatives, a CTA might pull in relevant functions from elsewhere in Government, such as support for university-based engineering research (from NSF), a technology extension effort, and perhaps aerodynamics programs (from NASA). Box V outlines one recently proposed agency. Beyond questions of size and scope, a CTA's effectiveness would depend heavily on four questions: 1) its mission; 2) project selection and monitoring; 3) the quality of its staff; and 4) intramural research.

Mission

The CTA's central mission would be to extend the time horizons of U.S. industrial R&D, and help fill some of the gaps in the Nation's technology base. More specifically, it would be responsible for supporting two rather different kinds of work. The first is *long-term, high-risk R&D* at pre-commercial stages, with the goal being relatively dramatic advances in technology. For example, candidate projects in the manufacturing area might include direct reduction steelmaking, or expert systems for shop-floor production scheduling. HTS examples could begin with three-terminal electronic devices, or integration of semiconductor and superconducting electronics.

The second area is *generic technology development*, which would typically be incremental.

Box V.—An Advanced Civilian Technology Agency, as Proposed in S. 1233

S. 1233—the Economic Competitiveness, International Trade, and Technology Development Act of 1987—reported by the Senate Governmental Affairs Committee in 1987. It was then incorporated in that house's omnibus trade bill, later to be dropped. S. 1233 is of interest here because of its provisions for an Advanced Civilian Technology Agency (ACTA), which would have been part of a new Department of Industry and Technology—the latter created through a major reorganization of Federal Government responsibilities.

The ACTA provisions in Title I, Part III of S. 1233 represent the closest that Congress has yet come to implementing some form of civilian DARPA.[1] Intended to support technology development and commercialization through contracts and grants, cost-shared with industry, the agency would give particular attention to risky, long-term projects. Technology-related functions transferred from the Commerce Department, including NBS, would stand alongside the ACTA (rather than becoming part of it). The bill authorized an ACTA budget big enough to make a difference—$80 million in the first year, rising to $240 million in the third year. Financial support to industry would be permitted through the stage of prototype development.

S. 1233 would provide for a high-level outside advisory board, but in most other respects leave agency operations up to the Secretary of the new department and his or her deputies. Report language calls for a small professional staff (35, initially), coming largely from industry, with considerable use of scientists and engineers on loan from the private sector.

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[1] As explicitly stated in *Economic Competitiveness, International Trade, and Technology Development Act of 1987: Report of the Committee on Governmental Affairs, United States Senate, To Accompany S. 1233*, Report No. 100-82 (Washington, DC: U.S. Government Printing Office, June 23, 1987), p. 10. In Section 122 of the bill, the agency is directed to coordinate its activities with those of both DARPA and NSF, the directors of which are to be members of its advisory board.

Many projects here would aim to reduce research to practice. The work would help many companies, but would rarely lead directly to proprietary advantage. Examples (again from manufacturing): nondestructive evaluation techniques, especially those suited to real-time operation as part of feedback control systems; small hand tools for mass production that are ergonomically designed for ease and speed of use (something that gets little attention in the United States compared to Japan and Europe). HTS examples include processing of the new ceramic materials, and magnet design and development for applications such as separation of steel scrap, or refining of ores.

Why is mission so important? Because it is a precondition for accountability. DARPA's mission creates discipline over the decisions of its staff and managers: only so long as DARPA can show that the work it funds will support future military requirements can the agency expect support from the Office of the Secretary of Defense and the relevant committees in Congress.

For a civilian agency, vague statements concerning commercialization or competitiveness will not do. Lack of agreement on mission is one of the reasons why none of the many bills introduced over the years to create a new technology agency has become law. Consensus on mission is critical for any agency with substantial budget authority—and given the size of the U.S. economy, and the needs for industrial technology, a CTA would have to have an annual R&D budget of $100 million or more for meaningful impact (DARPA's current budget is about $800 million).

Whatever form a CTA might take, it would never have a mission as clearly defined as that of DARPA or NASA. The overall goal—supporting commercial technologies in order to support the international competitiveness of U.S. industry—does not lend itself to neat and clean decisionmaking. Competitiveness is difficult to measure, harder to predict, and depends only partially on technology. Many people and many groups may view competitiveness as a legitimate goal. But as a practical matter, it would not be possible to judge the merits of a CTA's work—or evaluate the outcomes of completed R&D projects—by linking that work to competitiveness. The linkages are too loose, the causal connections often spanning many years. Even so, it should be possible to define the technological objectives of a CTA tangibly enough to provide a handle on mission.

Project Selection and Monitoring

With the charge of supporting commercial technology development, much of a CTA's budget would have to go for contracts with the private sector (including consortia), on a cost-shared basis. Some money might also flow to non-profit laboratories, and to universities through grants and contracts. But the point, after all, is to channel *direct* support to industrial technology, supplementing the many indirect measures the Federal Government already calls on.

A CTA would not be able to rely exclusively on review panels or outside experts to develop an overall strategy—a broad view of where resources should go—or for help in setting priorities. Outside experts, by definition, have a narrow view. The further science and technology advance, the greater the specialization among experts. The CTA would have to depend on the collective judgment of its own staff for strategy and priorities.

How about project-specific decisions? Money for private firms raises questions. The agency would have to choose projects on grounds that would be accepted as fair. Again, the answer begins with a competent staff, combined with merit review processes (box W).

The CTA Staff

Federal support for commercial technologies will always run the risk of devolving into little more than a program of subsidies for industry, with much of the money going to marginal projects. The primary guarantee against that danger is to staff the CTA with professionals who have the independence of judgment and the technical knowledge to make good decisions and stick to them.

To gain the respect of their industrial counterparts, CTA employees—technical specialists, program managers, administrators—would need a good grasp of market realities, as well as of industry's technical requirements. They would need to function as part of a peer group that includes industrial scientists, engineers, and R&D managers.

If the agency's managers were to provide exciting work, give employees substantial responsibilities, and maintain a selective and competitive personnel policy—more like DARPA or the Office of the U.S. Trade Representative than the Commerce Department—a CTA should have little trouble in assembling a capable staff. Finding people with strong technical credentials is relatively easy (American universities excel at deep but narrow training of engineers and scientists). Breadth and experience—industrial experience, in particular—are harder to find. Bringing in people from industry might require exceptions to normal civil service requirements.

Given that the Federal Government already employs many highly competent engineers and scientists, the CTA could begin by assembling a core staff borrowed from other agencies. Continued use of detailees would ensure a steady flow of fresh perspectives, while also helping with inter-agency coordination and technology transfer. Industry sabbaticals that sent CTA employees to the private sector for periods of 6 months to 2 years could help serve the same purpose (as suggested in ch. 4 for national laboratory employees).

Intramural R&D

Although most of its projects would be contracted to industry, it would also seem desirable for a CTA to carry out in-house R&D in its own facilities. This need not be a large-scale undertaking (say, 5 percent of the agency's budget). But it would allow staff members to keep their hands in. Some technical employees might rotate through the CTA's laboratories. Others could spend part of their time engaged in R&D more or less continuously.

Box W.—Project Review

When it comes to selecting projects for extramural funding, Federal mission agencies rely primarily on expert technical reviews. DoD conducts many of its technical reviews internally, but also looks to external bodies, permanent and *ad hoc*, on occasion.[1] NSF and NIH use outside review panels extensively, aiming at merit-based rankings reflecting the collective judgment of a group of recognized experts—the peer review model.

Not perfect, these processes can be criticized if the reviewers do not have appropriate qualifications (a problem particularly for internal agency reviews), or represent the conventional wisdom when the need may be to break the mold of ongoing research. Sometimes reviewers may favor their friends (true anonymity may be impossible in a specialized field). But the general approach has been widely accepted. It works, and—if applied appropriately—should work for sponsorship of industrial R&D by a CTA.

Once a broad agenda of R&D priorities had been set, the CTA could look to review panels for merit-based judgments, mixing outside engineers and scientists with the agency's own staff. The outside people would have to come from organizations without a direct stake in outcomes. There is no reason why a materials scientist working for an electronics firm could not give a fair review to the materials-related portions of HTS proposals on electrical machinery. Alone, such an individual would not have the expertise. In a group, he or she would contribute a useful perspective.

Contract monitoring, necessarily, would be the responsibility of the CTA staff. Agency employees would need to keep a critical distance from sponsored work, and be willing to cut off funding to companies that failed to perform (something DoD has difficulty doing on occasion).

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[1] The Advisory Group on Electron Devices, a longstanding committee of specialists from the three military services, industry, and universities, exerted considerable influence in shaping the Very High-Speed Integrated Circuit program. See "Federal Support for Industrial Technology: Lessons From VHSIC and VLSI," prepared for OTA by G.R. Fong under contract No. H38510, December 1987.

NSF uses criteria for rating proposals that include: technical competence of the proposed research, based on past achievements, as well as the details of the proposal; "intrinsic merit of the research," meaning the likely impacts on scientific advances; and relevance. See *Guide to Programs: Fiscal Year 1988*, NSF 87-57 (Washington, DC: National Science Foundation, 1987), p. ix. For a recent examination of scientific peer review, see *University Funding: Information on the Role of Peer Review at NSF and NIH*, GAO/RCED-87-87FS (Washington, DC: U.S. General Accounting Office, March 1987).

This activity would also force staff members to demonstrate that they can produce what they want others to produce—R&D that is relevant. The test is simple. If the intramural R&D is picked up and used by industry, the CTA staff has passed. Table 15 summarizes this and other features of a CTA.

Pitfalls

The primary difficulties for any agency charged with supporting commercial technologies are likely to be political rather than technical. The problems of defining an R&D agenda, and recruiting a staff with the right mix of skills and experience would be straightforward compared with the problems of establishing credibility within the broader system of U.S. policymaking. Like any Government institution that seeks to endure, a CTA would have to respect notions of democratic virtue. That requires—in addition to accountability—prudence in spending public funds, fairness in dealings with the private sector, and some degree of balance with respect to regional interests.[7]

For a CTA to become a reality, any proposal would have to satisfy constituencies having very different interests—some conflicting. The Frost Belt, seeking to rebuild its technological base and infrastructure, would no doubt want

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[7] E. Bardach, "Implementing Industrial Policy," *The Industrial Policy Debate*, C. Johnson (ed.) (San Francisco: Institute for Contemporary Studies Press, 1984), p. 103. The discussion following draws heavily on Bardach. Also see H. Heclo, "Industrial Policy and the Executive Capacities of Government," *The Politics of Industrial Policy*, C.E. Barfield and W.A. Schambra (eds.) (Washington, DC: American Enterprise Institute, 1986), p. 292; and *International Competitiveness in Electronics* (Washington, DC: Office of Technology Assessment, November 1983), ch. 12, especially pp. 475-482.

Table 15.—Desirable Features in a Federal Agency for the Support of Commercial Technology Development

Budget.—$100 million to $500 million annually in early years, exclusive of industry cost sharing, with 90 percent or more going for the support of R&D projects. These projects might be split roughly as follows:

- Industry, both single companies and consortia—80 percent
- Universities and non-profit research institutes—15 percent
- Internal agency projects—5 percent

Cost-sharing on industry projects at 40 to 60 percent seems appropriate to ensure that companies view the work as important.

Staff.—At the $500 million level, the agency would probably need about 250 professional employees. At any one time, about half the professional staff time would be devoted to intramural R&D—with technical employees expected to spend some fraction of their time, over a period of years, actively engaged in R&D.

Substantial use of detailees from other Federal mission agencies, as well as people on leave from universities and industry would be desirable. The agency's permanent staff members could also be expected to spend periodic tours in industry.

Intramural R&D.—At the $500 million level for the agency, 5 percent for intramural R&D means $25 million annually (and perhaps 100 full-time equivalent professionals). It would seem preferable to maintain a number of relatively small efforts, spread quite widely across the spectrum of industrial technologies; given that the primary function of intramural R&D is to maintain staff expertise, breadth would be essential. Even with half a dozen R&D areas, many staff members with other specialites would have to spend time in industrial laboratories to maintain hands-on R&D skills.

SOURCE: Office of Technology Assessment, 1988

parity with the Sun Belt; small business interests would probably begin lobbying for set-asides. Long before it opened its doors, an agency with the mandate to "restore U.S. technological competitiveness" would face pressures from companies in financial straits, seeking, for instance, relaxation in the CTA's requirements for cost-sharing.

Other threats to neutral allocation of CTA resources would be almost as quick to materialize. Perceived inequities between competing firms and industries would be all but impossible to avoid even with long-term R&D. CTA-funded R&D on magnetic separation of steel scrap would help minimills at the expense of integrated steel producers. Advances in magnetic levitation rail technology would threaten aircraft manufacturers and airline companies.

Such pressures could easily jeopardize the CTA's intended focus. Not only would distressed industries be pushing for a quick fix, a national emergency—an energy crisis, say—would bring calls for technological solutions. With high turnover likely in its political leadership—on average, assistant secretaries remain in Government for only 18 months—a CTA would be under constant pressure to show results. Pressures for immediate results would coexist with pressures to maintain funding for major projects, even if they proved flawed. Managers would be reluctant to admit mistakes, and—compared to their private sector counterparts—have less incentive to do so. Even flawed projects develop constituencies, moreover, ready to argue for continued funding.

In sum, a CTA—like any public institution—would have to win favor from enough well-situated constituents to continue its work. That is as it should be. The danger—and a very real one—is that, as an institution charged with spending money, a CTA would become just another forum for the distributive clashes that already consume Congress and much of the executive branch. Said one close observer of science policy, "We have the most highly-developed system of interest groups in the world, and they've discovered R&D."

CONCLUDING REMARKS

The Federal Government's responsibility for promoting technology is plainest in two cases: support for basic research, and investment in risky and speculative technologies. In both, substantial public benefits may coincide with meager private returns. The problem for U.S. technology policy is not only to find and support such R&D, but to stimulate industry to use the results in timely fashion.

Basic research continues to flourish under the present system of U.S. support for science and technology: when the breakthoughs in HTS occurred, the Nation had the resources and flexibility to mount a considerable effort in short order. To support advanced technologies, however, the United States has traditionally relied on the mission agencies—DoD, in particular. The approach reflects a philosophical distaste for government involvement in the economy, and also the belief that government cannot anticipate the needs of the marketplace; spinoffs, rather than direct financing, have supported many of the new technologies that American industry commercialized.

The approach worked well for several decades. But the world has changed. Military technologies have grown steadily more specialized, the defense sector more isolated from the rest of the economy. If DoD R&D funding was ever a cornucopia for U.S. industry, it is no longer. Second, other countries have caught up in technology. Today, both Japanese and West German firms spend higher proportions of their revenues on R&D than American firms. That spending has been one of the critical elements in their competitive success.

The emerging pattern in HTS seems much like that in microelectronics. Japanese firms are investing their own funds heavily. Government policies support their efforts. In the United States, only a small fraction of the Federal money for HTS finds its way into industry, and most of this will pass through DoD.

These and other indicators lead to the conclusion that a continuation of current policies for supporting commercialization of HTS will leave U.S. industry behind its strongest international competitors. The United States may continue to dominate the science of superconductivity, and might pioneer in commercial innovations. But the contest will eventually come down to engineering and manufacturing, where American industry has fallen down in recent years, and where the Japanese continue to improve.

OTA has analyzed two alternatives to the business-as-usual approach. One of the choices —creation of a Federal technology agency, with HTS as a piece of its territory—holds promise for the future. Such an agency might support industrial technology directly; many proposals have envisioned a kind of civilian DARPA, established to focus on R&D relevant on the civilian side of the economy. The pitfalls are not so much technical—maintaining a sound portfolio of projects—as political. A CTA would have to deal with the demands of distressed industries, depressed regions, and companies simply attracted by a pot of R&D money.

Whatever their merits, the alternatives under Strategy 3 cannot offer near-term support for HTS. The *ad hoc* measures outlined under Strategy 2 could. This approach —Federal cost-sharing of joint R&D—would be explicitly designed to promote an industry-centered agenda of long-term, high-risk R&D in superconductivity. Government's role—carried out through a working group on commercialization—would be as facilitator, as well as financier, helping to establish consensus on a research agenda, and securing cooperation from universities, Federal laboratories, and mission agencies. The three elements in Strategy 2 meet the needs summarized at the beginning of the chapter: diversity and continuity of Federal support; market-driven decisions; technology push complemented by market pull; low visibility; collaboration among industry, universities, and Government. In conjunction with ongoing activities in the mission agencies, they would substantially improve the odds on U.S. industry in the race to commercialize HTS.

Appendix A

Glossary

Coil/rail gun: Uses a rapidly changing magnetic field in a spiral coil (coil gun) or a linear conductor (rail gun) to accelerate a projectile via magnetic forces. Much greater velocities can be reached than are possible with gas expansion (as in a conventional gun).

COMAT, Committee on Materials: An interagency group under the Federal Coordinating Council for Science, Engineering and Technology chaired by the White House science advisor. COMAT's Superconducting Materials Subcommittee, chartered in June 1987, is comprised of program directors and other representatives of Federal agencies involved in superconductivity R&D.

Critical current density: The maximum value of the electrical current per unit of cross-sectional area that a superconductor can carry without reverting to the normal (non-superconducting) state. The critical current density drops as the temperature rises toward the transition temperature, and as the magnetic field increases.

CTA, Civilian Technology Agency: Several legislative proposals over the years would establish a Federal CTA focused on commercial technology development.

DARPA, Defense Advanced Research Projects Agency: A Defense Department R&D funding agency that gives most of its support to long-term, high-risk projects. Examples include artificial intelligence, and, currently, processing of high-temperature superconductors.

DPR, Domestic Policy Review of Industrial Innovation: A Carter Administration study of alternative Federal policies for stimulating technological innovation.

Electromagnetic launcher: See coil/rail gun.

ERCs, Engineering Research Centers: Cross-disciplinary research centers funded by the National Science Foundation at universities.

ETL, Electrotechnical Laboratory: This Japanese laboratory, administered by the Ministry of International Trade and Industry, has been involved in superconductivity research since the mid-1960s.

FCC, Fine Ceramics Center: A laboratory in Nagoya, Japan jointly funded by industry and the Ministry of International Trade and Industry. Participating firms send researchers to work at the facility, which opened in the spring of 1987.

Fiber-optics: Use of glass fibers to transmit light (produced by lasers) for telecommunications and computer networking. Optical fibers can carry much more information than electrical wires.

FLC, Federal Laboratory Consortium on Technology Transfer: A network of technology transfer officers from 400 Federal laboratories and eleven agencies for facilitating transfers of technology from the laboratories to industry. First set up informally in 1974, the FLC was given a statutory basis in the Federal Technology Transfer Act of 1986 (Public law 99-502).

HTS, high-temperature superconductor: Refers to materials—four classes of which have been discovered since 1986—with much higher transition temperatures than previously known superconductors. (See LTS.)

Intermetallic compound: Chemical compounds of nominally fixed composition, one or more elements of which are metals. Most intermetallic compounds —e.g., the superconductor niobium-tin (Nb_3Sn)—are brittle and therefore hard to work with.

ISTEC, International Superconductivity Technology Center: An organization for superconductivity R&D set up by Japan's Ministry of International Trade and Industry.

IUCRs, Industry-University Cooperative Research Centers: National Science Foundation program that provides seed grants for cooperative R&D at universities.

JJ, Josephson junction: Superconducting electronic devices that can be used to sense electromagnetic radiation and also as digital switches (hence as logic devices in computers).

°K, degrees Kelvin: The absolute scale of temperatures, with 0° K (-494° F) equal to absolute zero (a temperature that can be approached but never reached.

Logic chips: Integrated circuits consisting of arrays of gates each of which implements a Boolean function such as AND, OR, NOR, NAND. Computer processors are built from logic chips, as are many specialized digital systems.

LTS, low-temperature superconductor: Materials that become superconducting only when cooled to a few degrees above absolute zero. All superconductors discovered before 1986 were low-temperature materials, with 23° K (-418° F) the highest known transition temperature. (See HTS).

Magnetically-levitated train (maglev): Trains suspended and propelled by magnetic forces offer the prospects of much higher speeds than can be achieved by conventional wheel-on-rail technologies. A prototype superconducting maglev train in Japan (also called a linear motor car) has achieved speeds of over 300 miles per hour.

Magnetometers: Sensors which measure magnetic field strength. Because magnetic fields accompany so many physical phenomena, magnetometers—including ultrasensitive versions made from superconducting devices—have many uses. (See JJ, SQUID.)

Magnetic resonance imaging (MRI): Refers to equipment and techniques used in medical diagnosis for imaging the soft tissues of the body. MRI systems often use superconducting magnets.

MCC, Microelectronics and Computer Technology Corp: A joint venture that conducts R&D. MCC's program to develop and evaluate electronic applications of HTS had 13 participants as of the spring of 1988.

MITI: Japan's Ministry of International Trade and Industry.

Monbusho: Japan's Ministry of Education, which supports university research.

MRLs, Materials Research Laboratories: Now supported by the National Science Foundation, several of the MRLs are conducting research on HTS.

Multicore Project: Established by Japan's Science and Technology Agency to link nine laboratories and government organizations working on HTS with one another and with industry.

NBS: National Bureau of Standards of the U.S. Department of Commerce.

New Superconductivity Materials Research Association: Generally called the "superconductivity forum," this association was set up by Japan's Science and Technology Agency. It provides workshops, symposiums, and other opportunities for interaction among corporations, universities, and national laboratories.

NSA: U.S. National Security Agency.

NSF: U.S. National Science Foundation.

1-2-3 superconductor: One of a new class of high-temperature superconductors, typified by yttrium-barium-copper-oxide and called 1-2-3 because of their generic chemical formula: $RBa_2CO_3O_{7-x}$, with R almost any one of the rare-earth elements.. Much of the research on the new superconductors has focused on the 1-2-3 materials, which typically have transition temperatures above 90 K.

OSTP, Office of Science and Technology Policy: Headed by the White House science advisor, OSTP is part of the Executive Office of the President.

Perovskite: Refers to the crystal structure shared by the 1-2-3 and other high-temperature superconductors.

Rail gun: See coil/rail gun.

RAM chips: Integrated circuits that provide random access memory for computers and other digital systems.

SBIR, Small Business Innovation Research: A Federal program, in operation since fiscal 1983, which requires Federal agencies to set aside a small percentange of extramural R&D budgets for contracts with small businesses.

SDI, SDIO: The Strategic Defense Initiative, and the Defense Department organization that runs it.

Sematech: An R&D consortium, financed by 14 member companies (as of the spring of 1988) and the U.S. Government, established to pursue improvements in semiconductor manufacturing technologies.

Signal-to-noise ratio: An important parameter for sensors, the signal-to-noise ratio compares the signal the sensor is intended to measure with background noise (one source of which is thermal, rising with temperature).

SMES, superconducting magnetic energy storage system: A coil or solenoid of superconducting wire in which an electric current can circulate, storing energy until needed for purposes such as feeding an electric utility grid or powering a free-electron laser.

SQUID, superconducting quantum interference device: A very sensitive instrument, built with Josephson junctions, used to detect magnetic signals.

STA, Science and Technology Agency: Under the Prime Minister's Office in Japan.

S&T centers, Science and Technology centers: Multidisciplinary centers proposed for funding by the National Science Foundation.

Stevenson-Wydler Act: The Stevenson-Wydler Technology Innovation Act of 1980 (Public law 96-480, as amended), placed increased emphasis on technology transfer from the Federal laboratories. The 1986 Federal Technology Transfer Act (Public law 99-502) amended the 1980 act to provide (among other things) more emphasis on cooperative research between federally operated laboratories and industry.

Superconductivity: Total loss of resistance to direct electrical currents.

Superconducting magnet: An electromagnet wound with superconducting wire. Essentially all the power consumed goes for refrigeration to keep the coil windings below their superconducting transition temperatures.

Three-terminal electronic device: One which, like a transistor, can amplify a signal substantially. (See two-terminal device.)

Transition temperature: The highest temperature at which a material becomes a superconductor, also known as the critical temperature. The transition temperature drops as the magnetic field and current density increase.

Two-terminal electronic device: one which, like a Josephson junction, can serve only as a weak amplifier. (See three-terminal device.)

URI, University Research Initiative: A Department of Defense program, started in 1986, intended to support university capabilities in research, and training of scientists and engineers, in disciplines important for national defense.

VHSIC program: An R&D program begun by the Department of Defense in 1979 to develop advanced integrated circuits for military systems.

VLSI program: Joint government-industry R&D effort in Japan for developing very large-scale integrated circuits (VLSI), in existence from 1976 to 1980.

X-ray lithography: Creation of patterns for fabricating integrated circuits using X-rays. Because X-rays have shorter wave lengths than visible light, they can produce finer patterns, hence denser circuits.

Appendix B

The Technology of Superconductivity[1]

Electric currents travel through superconductors with no resistance, hence no losses (provided the current is steady—alternating currents meet resistance even in superconductors). When current flows in an ordinary conductor, say a copper wire, some power is lost. In a light bulb or an electric stove, resistance creates light and heat, but in other cases the energy is simply wasted. With no resistance, magnets wound with superconductors can create very high fields without heating up and dissipating energy. Motors and generators with superconducting windings could be smaller, lighter, and more efficient than those built with copper. Very high magnetic fields might be used to fire projectiles, to float molten metal in a steel mill, to levitate trains.

Table B-1 lists some of the possible applications. During the 1960s, low-temperature superconductors (LTS)—specially developed metal alloys like niobium-titanium—came into use in specialized applications such as magnets for scientific research. Some current LTS applications—e.g., ultrasensitive magnetic field detectors—will not be superseded by high-temperature superconductivity (HTS) (because of higher thermal noise at higher temperatures). Other applications, possible but not practical with LTS, could become much more attractive with HTS.

As table B-1 indicates, superconductors not only banish electrical losses, and provide the basis for very sensitive detectors of magnetic fields and other radiation, but can also be used to produce the fastest possible electronic switching devices. In addition, superconductors exclude magnetic fields, which means they can be used as radiation shields. (The exclusion of external magnetic fields is termed the Meissner effect, after the physicist who discovered the phenomenon in 1933.)

Many of the applications listed in table B-1 have been goals for engineers and scientists since superconductivity was discovered early in the century. Along with powerful magnets for a variety of purposes, prototype generators, electrical transmission lines, and computer chips have all been made, operating in most cases at liquid helium temperatures (about 4° K, or 4 degrees above absolute zero, figure B-1). But the very low temperatures have been a barrier for many of the applications possible in principle.

High-temperature superconductors and liquid nitrogen cooling (77° K, figure B-1) would bring relatively modest improvements in costs, system complexity, and practicality for most of these applications. Liquid nitrogen temperatures, after all, are only high compared with the near-absolute zero of liquid helium. Where system designs already incorporate LTS—e.g., magnetic resonance imaging (MRI, ch. 2)—it might not pay to change over simply to take advantage of HTS. In applications where unattended operation is desirable—e.g., military surveillance or geophysical exploration—the much reduced boil-off rate of liquid nitrogen would be a major advantage. HTS also holds obvious attractions for applications in space; beyond low-Earth orbit, passive cooling may suffice to maintain superconductivity in the new materials.

Nonetheless, it is quite possible that continued R&D will bring new applications of HTS that cannot yet be anticipated. Superconductivity at room temperature, moreover, would be truly revolutionary. Compact and efficient small motors and actuators, for example, could find uses ranging from household products and automobiles to machine tool drives and power-packs for replacing aircraft hydraulic systems.

Superconductivity

Above its transition (or critical) temperature, a superconductor exhibits electrical resistance like any other material. Below the transition temperature, the material has zero resistance to direct current (DC): a steady electric current will circulate in a superconducting coil forever, so far as anyone knows. Variation in the flow of current does lead to electrical losses; thus a superconductor dissipates energy when turned on or off, or when carrying an ordinary alternating current (AC losses). However, these losses are much less than those in a good normal conductor (e.g., copper) at the same temperature.

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[1] Much of the material in this appendix is drawn from "Superconductive Materials and Devices," Business Technology Research, Wellesley Hills, MA, September 1987; and "Technology of High Temperature Superconductivity," prepared for OTA by G.J. Smith II under contract No. J3-2100, January 1988. Also see *Physics Today*, Special Issue: Superconductivity, March 1986; A.P. Malozemoff, W.J. Gallagher, and R.E. Schwall, "Applications of High-Temperature Superconductivity," *Chemistry of High-Temperature Superconductors*, ACS Symposium Series 351, D.L. Nelson, M.S. Whittingham, and T.F. George (eds.) (Washington, DC: American Chemical Society, 1987), p. 280; "Research Briefing on High Temperature Superconductivity," Committee on Science, Engineering and Public Policy, National Academy of Sciences, Washington, DC, 1987.

Table B-1.—Representative Applications of Superconductivity

Large-scale passive:
- *Shields, waveguides*
  - Superconductors screen or reflect electromagnetic radiation, possible applications range from coating of microwave cavity walls to protection from the electromagnetic pulses of nuclear explosions
- *Bearings*
  - Repulsive forces created by exclusion of magnetic flux make non-contact bearings possible

High-current, high-field:
- *Magnets*
  - Medical imaging
    - LTS magnets widely used in commercial systems
  - Scientific equipment
    - LTS magnets used in fusion experiments and particle accelerators
  - Magnetic separation
    - Possible uses include separating steel scrap, purifying ore streams, desulfurizing coal, and cleaning up stack gases. At least one LTS magnet is in current use for purifying Kaolin clay
  - Magnetic levitation
    - Levitated trains have been extensively studied, with prototypes in Japan and Germany
  - Launchers, coil/rail guns
    - Electromagnetic launching systems can accelerate objects to much higher velocities than gas expansion, possible applications range from small guns for military purposes to aircraft catapults and rapidly repeatable Earth satellite launching
  - Other
    - Powerful magnets could eventually find a very wide range of uses. Examples: compact synchrotrons for lithographic processing of integrated circuits, growth of the crystals for integrated circuits (a strong magnetic field yields more nearly perfect wafers of silicon and other semiconductor materials), MHD (magnetohydrodynamic) systems for energy conversion. MHD thrusters might also be used in place of propellers to drive ships and torpedos
- *Other static applications*
  - Electric power transmission
    - Prototypes of LTS underground lines have demonstrated feasibility, but such installations are not cost-effective (compared with overhead high-tension lines) at present
  - Energy storage
    - Solenoids wound with superconducting cable could store electrical energy indefinitely as a circulating current, in addition to utility applications (e.g., load leveling), superconducting storage could find uses in military systems (e.g., pulsed power for large lasers). Cheap and reliable superconducting energy storage would eventually find many other applications
- *Rotating machinery*
  - Generators
    - A number of LTS prototypes have been built to investigate possible electric utility applications
  - Motors, motor-generator sets
    - Used in conjunction with a superconducting generator, a superconducting motor could be an efficient alternative to mechanical power transmission for applications such as ship and submarine drives, railway locomotives, and perhaps even for helicopters. Sufficiently low costs would open up many industrial applications

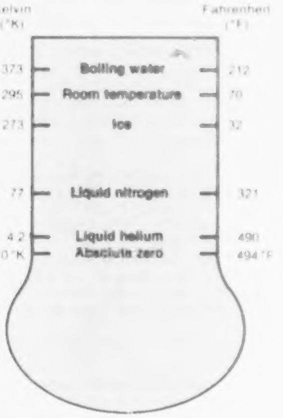
Electronics:
- *Passive*
  - Superconducting wiring (interconnects) for computers, on-chip or between chip, could help increase processing speed
- *Sensors*
  - SQUIDs (superconducting quantum interference devices) made from Josephson junctions (JJs) are the most sensitive detectors of electromagnetic signals known, applications range from detecting neural impulses in the human brain to geophysical exploration, detection of submarines in the deep ocean from airplanes or, potentially, from space, and nondestructive inspection
- *Digital devices*
  - JJs can also be used for digital switches, opening up such applications as computer logic and memory, competitive three-terminal devices with substantial gain may eventually be developed, combined semiconductor-superconductor devices or systems also hold many attractions
- *Other devices*
  - Analog/digital converters, voltage standards, many types of signal processors, and microwave mixers can all be designed, in principle, with superconductors, some of these applications (e.g., voltage standards) have been reduced to practice with LTS JJs

NOTE: This list is based on known properties of known materials; it is not exhaustive, even for existing, well understood low-temperature superconductors.

SOURCE: Office of Technology Assessment, 1988.

Until 1986, the highest known transition temperature was 23° K (in niobium-germanium). Then two IBM scientists in Zurich discovered a new class of materials that showed superconductivity at 35-40° K (see box C, ch. 2). Shortly thereafter, the compositions now termed the 1-2-3 superconductors were found, with transition temperatures in the vicinity of 95° K. The first of the 1-2-3 materials announced contained yttrium, barium, and copper oxide. More recently, copper-oxide ceramics containing bismuth or thallium have been found. These have superconducting critical temperatures in the range, respectively, of 110° K and 125° K. In April 1988, the first HTS compositions containing no copper were announced, with transition temperatures up to 30° K.

Figure B-1.—Temperature Scales


Kelvin (°K)
Fahrenheit (°F)
373
Boiling water
212
295
Room temperature
70
273
Ice
32
77
Liquid nitrogen
321
4.2
Liquid helium
490
0°K
Absolute zero
494°F

SOURCE: Office of Technology Assessment, 1988

A major advantage of the new materials, of course, is the potential for simpler, less expensive cooling. For technical reasons, superconductors must be operated well below their transition temperatures—as a rule of thumb, at half to three-quarters the transition value. In fact, then, liquid nitrogen temperatures will be marginal for the 1-2-3 compositions (although the practical advantages of operating in the range of 40° K rather than 4° K can be great). If the more recently discovered ceramics, with critical temperatures of 110° K and up prove to have otherwise useful properties, liquid nitrogen cooling will almost certainly prove adequate.

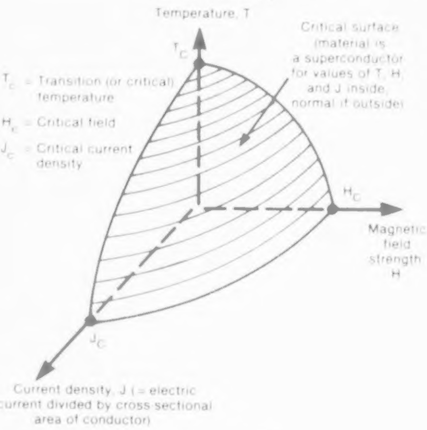
All the HTS compositions so far discovered are ceramics, rather than metals. They are new materials, poorly understood. All ceramics are brittle; they require processing and fabrication methods very different from metals and alloys.

Superconducting Properties and Behavior

In 1911, Heike Kamerlingh-Onnes, a Dutch physicist, made the astounding discovery that mercury lost all electrical resistance at 4° K. Earlier, Kamerlingh-Onnes and his research group at the University of Leiden had developed cryogenic refrigeration equipment capable of reaching these very low temperatures. In 1913, Kamerlingh-Onnes found that lead became superconducting at 7.2° K. At this point, the Leiden group built a superconducting magnet with lead windings, only to find the lead reverting back to its normal state when the magnetic field reached a few hundred gauss—a severe limitation on practical use, given that a common kitchen magnet creates a field of about 1,000 gauss. (The average magnetic field of the Earth is one-half gauss; the windings in electric motors create fields of about 10,000 gauss.)

Critical Properties.—Later it was learned that maintaining the superconducting state requires that both the magnetic field and the electrical current density, as well as the temperature, remain below critical values that depend on the material. Figure B-2 shows this schematically, while table B-2 gives the critical values of temperature and field for a number of superconducting materials. Practical applications, in general, require that *both* the transition temperature and the critical current density be high; in some cases, relatively high magnetic fields are necessary as well.

Figure B-2.—Dependence of the Superconducting State on Temperature, Magnetic Field, and Current Density


Temperature, T
T_C
Critical surface (material is a superconductor for values of T, H, and J inside, normal if outside)
T_c = Transition (or critical) temperature
H_c = Critical field
J_c = Critical current density
H_C
Magnetic field strength H
J_C
Current density, J (= electric current divided by cross-sectional area of conductor)

SOURCE: Office of Technology Assessment, 1988

Table B-2.—Critical Values for Superconducting Materials

	Temperature (degrees Kelvin)	Magnetic field (gauss)	Current density [a] (amps per square centimeter)
Aluminum	1.2	105	
Mercury	4.2	410	
Lead	7.2	800	
Niobium	9.2	0.4×10^4 [b]	
Niobium (75%)-titanium (25%)	10	14×10^4 [b]	~10^5 [c]
Niobium-tin	18	23×10^4 [b]	~10^7 [c]
1-2-3 ceramic ($YBa_2Cu_3O_{6.9}$)	93	100 + $\times 10^4$ [b]	10^3 to >10^6 [d]

[a] At zero magnetic field

[b] Upper critical field (Type II superconductor)

[c] At 4.2° K

[d] At 77° K. The highest values are reached with oriented single crystal films.

SOURCE: Office of Technology Assessment, 1988

By the late 1930s, scientists had distinguished Type I and Type II superconductors. Type I materials, in which the phenomenon had first been studied, shift abruptly to their normal state above the critical magnetic field. Type II superconductors exhibit a mixed state between two values of magnetic field, the lower and upper critical fields. The new HTS materials show Type II behavior,-with extremely high critical fields (table B-2)—indeed, so high in the 1-2-3 compositions that simply measuring them has proven very difficult.

As Kamerlingh-Onnes found with his lead-wound magnet, Type I superconductors have critical fields too low to make useful magnets. While Type II materials have much higher critical fields, the new HTS materials have proved to have disappointingly low values of the third critical parameter—the current density (table B-2).

Raising allowable current densities in the 1-2-3 ceramics from the values found initially in polycrystalline samples (consisting of many grains, randomly oriented)—below 10^3 amps per square centimeter—quickly became a major research target. For most applications, improvements of 100 times or more—to the range of 10^5 or 10^6 amps per square centimeter—will be needed. This is important not only for high-power applications; electronic devices carry small currents, but current densities are high because cross-sectional areas are microscopic.

Not a fundamental limitation, the low current densities are materials processing problems (critical current density depends on the microstructure of the material, hence on its processing). Many years of R&D were needed to raise the critical values for Type II superconductors like niobium-titanium to the values shown in table B-2. Similar effort lies ahead for the new HTS materials; so far, progress has been most rapid in thin films.

From the 1930s to the 1980s.—The 1930s and 1940s saw a good deal of progress in the cooling and refrigeration systems needed to reach very low temperatures, spurred in part by wartime needs for liquid oxygen.[2] After the end of the Second World War, the newly formed Office of Naval Research established a major research program in low-temperature physics. One consequence—rapid improvements in the technology for producing liquid helium—made experimental research in superconductivity much easier.

Federal support during the 1960s included development of very powerful superconducting magnets, principally for conducting experiments in high-energy physics and nuclear fusion, as well as exploratory studies of possible electronic applications of superconductivity. The first of the current generation of conductors, niobium-tin, was developed during the early 1960s. A brittle intermetallic, very difficult to work with, niobium-tin found little use. Within a few years, almost all LTS magnets were being wound with niobium-titanium—more ductile, though with a lower critical field.

With steady progress in processing techniques, niobium-titanium conductors could be fabricated as braided cables each containing thousands of very fine filaments. Small filaments—less than the diameter of a human hair—reduce the AC losses stemming from variations in current, and have other highly desirable properties for magnet applications. These filaments are embedded in a copper matrix, capable of carrying the full current in the event of an accidental loss of superconductivity. The conductors must be flexible enough for winding, and mechanically strong because the magnetic field creates very high forces. Many years of R&D have led to steady improvements in LTS magnets.

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[2] "Government's Role in Computers and Superconductors," prepared for OTA by K. Flamm under contract No. H36470, March 1988, pp. 43ff.

The discovery of the Josephson effect in the early 1960s opened up a new class of possible applications in electronics. Josephson junctions (JJs), made with a thin insulating layer (a matter of a few atomic diameters) separating two superconductors, can act as very fast electronic switches—comfortably exceeding the fastest semiconductor devices. Because they dissipate so little power—about 1,000 times less than semiconductors—JJ electronics can be more tightly packed, which also contributes to speed.

Theory

Theoretical work had likewise moved ahead, culminating in 1957 in the Bardeen-Cooper-Schrieffer (BCS) model, for which a Nobel Prize was later awarded. The BCS theory explains superconductivity in terms of interactions between electrons (which carry current) and phonons (atomic vibrations). Under normal conditions, the electrons collide with atoms, leading to electrical resistance and energy loss. In the superconducting state, electrons move in coordinated fashion without any collisions.

While the BCS model explains superconductivity in the old materials, so far HTS has baffled the theorists. Until 1986, most of those working in the field had agreed that BCS superconductivity at temperatures above about 30° K was impossible. Given the recent experimental results, the theoretical community has been scrambling to extend the BCS model or find some new explanation.

Although the Josephson effect was first predicted theoretically, and later confirmed by experiment, theory gave little or no guidance in the search for LTS materials with higher transition temperatures. Thus the situation is really no worse for HTS. Looking for materials with higher transition temperatures remains largely a matter of trial-and-error, guided by intuition—laboratory research that is time-consuming and expensive.

Processing and Fabrication

Tailoring HTS materials for applications either in electronics or where high currents and/or fields are needed (e.g., electric power) will entail design and processing at size scales from the atomic level on up. Engineers and scientists engaged in applications developments, as well as materials processing, will have to concern themselves with electronic structure (energy gaps), crystal structure (the arrangement of atoms in the material), microstructure (grain boundaries), and the fabrication of films, filaments, tapes, and cables.

The HTS ceramics are not only brittle, and chemically reactive, but highly anisotropic—meaning that properties vary with direction within a grain of the material. The 1-2-3 ceramics, for instance, show differences of as much as 30:1 in critical current density depending on grain orientation. Some of the current density limitations can be traced to anisotropy, but grain boundaries seem to be the primary culprit.

Some of the processing and fabrication techniques familiar from work with electronic and structural ceramics hold promise for the new superconductors. Bulk samples of HTS material can be made by hot pressing, extrusion, and tape casting, among other methods. The anisotropy in the 1-2-3 materials has led many research groups to seek processes for aligning the grains—e.g., extruding a slurry of single crystals in a high magnetic field to create a tape. Semiconductor fabrication techniques, likewise, can in some cases be adapted for making thin films.

Past work on niobium-tin, a brittle intermetallic compound, may also hold lessons for HTS materials, which, like other ceramics, cannot deform plastically. Because they break easily and without warning—like glass, being very sensitive to small imperfections (hence the scribed lines used to "cut" glass)—practical applications may require specialized in-situ processing, as well as careful design to minimize strain. Magnets wound with niobium-tin are made starting with strands of niobium in a copper-tin alloy matrix—flexible and ductile. Heat treatment after the wires have been drawn and wound into coils causes the tin to combine with the niobium, forming the superconducting compound, with its vastly different properties. Some R&D groups have pursued similar processes for ceramic superconductors.

Progress has been faster with thin films, which can be created via a wide range of well-known techniques—e.g., sputtering, and evaporation by molecular or electron beams. Finding good substrates on which to deposit the HTS layer has been the primary problem. The HTS compounds react chemically with many otherwise desirable substrate materials, including those used for integrated circuits (silicon, sapphire). Strontium titanate gives high current densities compared to other choices, but is expensive and has otherwise undesirable properties. Silicon would be ideal as a step toward combining semiconductor and superconducting electronics. While the temperatures so far required for creating the proper HTS composition have posed difficulties, many research groups have been working on the problems, with encouraging results.

Applications

Much of the excitement over HTS has been stirred by speculation concerning such possible applications as low-loss electric power transmission or magnetically levitated trains. In some of these cases, commercialization will depend more on system costs and progress in competing technologies than on the specifics of HTS. Both transmission lines and levitated trains have been demonstrated with LTS materials. Superconducting transmission lines, which must be run underground because of the cooling requirements, may eventually prove cost-effective relative to conventional underground transmission; thus far, however, these applications have not moved out of the test stage. Maglev trains could be built by the end of the century in Japan and West Germany. Competing technologies sometimes present a moving target for superconductivity: after more than 10 years of R&D aimed at a Josephson computer, IBM concluded that competing semiconductor technologies were improviag rapidly enough that its approach to JJ computer elements would probably not bear fruit.

More than likely, then, 5 to 10 years of R&D lie ahead before many applications of HTS emerge. Those that come earlier are likely to be highly specialized—perhaps in military systems, perhaps targeted on very demanding civilian needs (for example, Hypres' very high-speed data sampler, which incorporates LTS electronics). The ongoing R&D will involve:

1. Basic research, both theoretical and experimental, aimed at explaining HTS, at finding new materials and exploring their properties, and at understanding structure-property relationships.
2. Applied research, focused particularly on development of processing methods and optimization of material properties through manipulation of processing variables. A great deal of R&D will be needed before routine production of tapes and multifilamentary conductors could begin, with substantial improvements in critical currents an early step. Josephson junctions for electronics will also be difficult to reduce to practice.
3. Applications engineering (for HTS)—e.g., development of prototype chips containing many JJs—including extensive testing under realistic operating conditions (environmental exposure, thermal cycling, mechanical vibrations, electrical surges, loss of temperature control). Joining techniques for conductors will be needed; so will repair methods.
4. Process engineering—manufacturing methods for routine (rather than laboratory) production. Problems here will include yields and reliability in superconducting circuits, and methods for producing long continuous lengths of superconducting cable. Inspection, testing, and quality control procedures will need a good deal of attention.
5. Systems engineering—design, development, and demonstration of applications in which superconducting components are integrated into such end products as computers, electrical generators, and coil or rail guns. For instance, without further progress in transition temperatures, HTS interconnects in computers will require cooling to liquid nitrogen temperatures. Fortunately, these temperatures also offer performance advantages for semiconductor chips.

Many of these activities can go forward in parallel. In some cases it makes sense to proceed sequentially. For instance, applied research aimed at increasing current density can and should proceed in conjunction with process R&D, because processing affects microstructure, and microstructure affects current density. But work on production scale-up must wait until the effects of processing variables can be reasonably well understood. On the other hand, research intended to discover whether a particular processing technique—e.g., laser annealing—compromises some properties will be needed early.

High-Current, High-Field Applications

Magnets.—Most past applications of superconductivity have involved the design and construction of powerful magnets wound with LTS materials and cooled with liquid helium. Such magnets have been used in scientific experiments (e.g., the Tevatron, ch. 2), and in MRI. Learning to design and build magnets helps with more demanding applications, such as rotating machinery.

Almost all the power consumed by a superconducting magnet goes to operate the cooling system. For a big magnet wound with copper, resistive losses far outweigh the refrigeration costs for an equally powerful LTS magnet. Indeed, large copper-wound magnets need their own cooling systems just to carry off the heat generated through resistance. The cost comparison below, for a bubble chamber magnet at Argonne National Laboratory—a typical early scientific application—shows that a conventional magnet would cost five times more to operate:[3]

Annual operating costs (thousands of dollars)

	Superconducting magnet (actual)	Conventional magnet (estimated)
Electrical power	$17.5	$550
Cooling	81.3	4
Maintenance	5.2	?
	$104	$554 +

Superconducting magnets have other advantages compared with conventional magnets. Stability is easier to achieve, for instance. In a conventional magnet, the field strength varies as the windings heat up and expand. The stability characteristics of LTS magnets give them advantages both in scientific apparatus and in MRI.

With LTS magnet technology well in hand, HTS designs will have to perform at least as well (in terms of characteristics such as stability) before their simpler cooling systems and lower operating costs will make them competitive. Fabricating the conductors will be difficult. Stable operation and protection against overheating in the event of refrigeration failures require multifilamentary cables, just as for LTS, with filament diameters of a few microns.[4] Given the brittleness of the new ceramics, methods for producing filaments and for fabricating cables are not yet in sight.

Once HTS wire and cable become available, applications-specific requirements will come to the fore. MRI, for example, while requiring highly stable fields for good image quality, does not otherwise make heavy demands on the magnet system. Still, joining methods that eliminate resistive imperfections will be needed for image quality comparable with that already achieved using LTS.

MRI systems are expensive, and savings from simpler cooling will not make that much difference for commercial competition. Magnetic separation is another story. Here, for instance, cheap but powerful magnets could be used to sort scrap metal for recycling, in refining ores, purifying chemicals, removing sulfur from pulverized coal, and cleaning up waste water. In all these applications, cost, reliability, and ease of use (including maintenance) by a largely blue-collar labor force become significant design considerations. Design considerations for maglev trains likewise include cost, reliability and longevity, and safety. But the political and economic questions loom even larger than for, say, desulfurizing coal. In the United States, investments in fixed-rail transportation would have to clear obstacles ranging from opposition by airlines to high costs for rights-of-way. In Japan, where the needs and constraints differ, R&D on HTS-based maglev is much more likely to go forward.

Electric Power and Utility Applications.—Magnets have no moving parts. Technical complexities grow in electrical machinery, and in the entire range of electric utility applications. Transformers, for example, would demand more attention to AC losses than magnets, while superconducting transmission lines will almost certainly have to go underground, so long as refrigeration is required. Undergound lines are costly, although already in use in many urban areas. Still, the over-riding design requirement is reliability. Utilities are quite willing to trade off higher operating costs against lower probability of failures and down-time. A disabling failure, after all, can lead, not only to a blackout, but to an ongoing need to purchase power from other suppliers until repairs have been completed.

In general, HTS-based generators will need conductors similar to those for magnets. Dynamic forces, however, will add to static forces, while cooling also becomes more difficult. Large conventional generators already have efficiencies greater than 98 percent. Superconducting field windings can increase this to more than 99 percent. In a large machine, an improvement of 0.5 percent to 1 percent in efficiency can be significant—reducing the losses by half—while superconducting generators have the additional advantage (for utility applications) of increasing network stability (they are less sensitive to shifts in electrical load).

Worldwide, at least two dozen LTS generator R&D projects have been undertaken since the middle 1960s, but none has gone beyond construction and testing of a prototype. Utilities will have to be convinced that such machines offer reliable service over periods of many years before investing; HTS will not affect the economics much compared to LTS, and, lacking even the experience base of

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[3] P.J. Reardon, "High Energy Physics and Applied Superconductivity," *IEEE Transactions on Magnetics*, vol. MAG-13, 1977, p. 705. This magnet, for Argonne's 12-foot bubble chamber, draws 1800 amperes, producing a field of 1.8 × 10^3 gauss. The cost figures assume 140 operating days per year.

As another example, a magnetic separator for purifying Kaolin clay (table B-1) consuming 270 kilowatts (kW) if built with a conventional magnet, plus another 30 kW for cooling the magnet, could today be replaced with an LTS magnet that needed no more than 60 kW, all for refrigerating the windings.

[4] *Report of the Basic Energy Sciences Advisory Committee Panel on High-Tc Superconducting Magnet Applications in Particle Physics*, DOE/ER-0358 (Washington, DC: Department of Energy, December 1987), pp. 9-12.

LTS systems, the new materials have an added hurdle to overcome. Energy storage rings—with no moving parts, and tolerable failure modes—will almost certainly come first.

Other Electrical Machinery.—For non-utility applications, characteristics other than efficiency and reliability come to the fore: superconducting machines promise to be smaller and lighter than conventional motors and generators by half and more. These are the attractions for ship propulsion, where a superconducting generator driving a superconducting motor could eliminate the gearing and shafting between turbine (or other prime mover) and propeller. With much more freedom in packaging, nuclear submarines could carry more weapons (or be smaller). So could surface ships. Submarines might also prove quieter, perhaps even faster. Moreover, with the motor/generator set(s) providing speed control (and reversing), efficiency during part load operation would rise (the turbine can run at its optimum speed).

As table B-1 indicated, other, more cost-sensitive, applications for motor/generator sets might also open up at some point. And of course, given high enough operating temperatures, the many large electric motors used throughout industry (ranging from pump, fan, and blower drives to machine tools and rolling mills) would be candidates for replacement.

Electronics

From the beginning, Josephson junctions have been the basis for many superconducting electronic devices. SQUIDs—superconducting quantum interference devices, simple circuits incorporating JJs—have extremely high sensitivity levels, which have led to a considerable range of practical uses for LTS SQUIDs. The Josephson effect can also be exploited for computer logic and memory; although a number of practical problems stand in the way, JJs could in principle replace semiconductor chips in powerful digital processors (box J, ch. 3).

Sensors.—SQUIDs can detect the very faint signals produced by the human heart (10^{-6} gauss) and brain (10^{-9} gauss). These simple circuits can also measure a wide variety of other electromagnetic signals (anything with an associated magnetic signature from DC up to microwave frequencies). SQUIDs are about 1,000 times more sensitive than the next best magnetic field detectors. They can sense the disturbances in the Earth's magnetic field caused by a submarine deep in the ocean, or the field distributions caused by geologic formations holding oil or mineral deposits. Requiring, in simplest form, only one or two JJs (rather than the large numbers required in computer applications), LTS SQUIDs—typically fabricated from niobium—are now made routinely.

To minimize thermal noise, SQUIDs should be operated at the lowest possible temperature, and in any case at less than half to two-thirds of the superconducting transition temperature. At liquid nitrogen temperatures, for instance, sensitivity will be 20 times poorer than at liquid helium temperature. Even so, an HTS SQUID would still be a more sensitive magnetic field detector than any of the alteratives except an LTS SQUID. If they can be built successfully, HTS SQUIDs will quickly find a considerable range of applications (though none of these are likely to be high-production-volume applications).

Computers and Other Digital Systems.—JJ-based electronic devices promise switching speeds 10 times faster than the very best compound semiconductors. Because the energy losses are several orders of magnitude smaller, JJ-based integrated circuits could be packed much more densely. However, the practical problems of making JJ-based chips far exceed those of SQUIDs.

Even if the practical problems were solved, Josephson computers might not be commercialized. The competing technologies extend well beyond silicon and gallium arsenide chips: a good deal of R&D has been going into alternative computer architectures such as massively parallel processors. Much of this work seeks increases in processing power without major advances in components. Still, faster chips will always promise faster machines. But, in a further contrast with SQUIDs—which are the most sensitive magnetic field detectors known—the theoretical limits of JJ-based logic devices fall well short of what might eventually be possible, for example, using optical switching. Thus the window of opportunity for JJ-based computing may never open. (It may never open for optical computing, either.) On the other hand, advances in device design—and, in particular, a practical three-terminal device that would erase the primary drawback of JJ chips, low gain—could open a broad new frontier.[5] It is simply too early to say.

R&D in the United States and Japan on LTS-based JJ computing illustrates some of the problems that designers of HTS logic and memory would face. IBM was able to build logic chips with 5,000 junctions reliably, but had trouble with cache memory.

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[5] S.G. Davis, "The Superconductive Computer In Your Future," *Datamation*, Aug. 15, 1987, p. 74.

(Fast logic does no good without fast cache memory for support.) IBM's prototype memory chips, with over 20,000 JJs, proved susceptible to errors caused by slight variations in control current—a good example of the kind of problem that a 3-terminal device would help solve. More recently, Japanese companies have built several kinds of LTS chips incorporating niobium JJs. Fujitsu's 4-bit microprocessor, 25 times faster than a similar silicon chip, and 10 times faster than a gallium-arsenide microprocessor, consumes only 0.5 percent as much power as either. NEC has produced a 1,000 bit dynamic memory, containing 10,000 JJs; access time is a factor of 200 better than for silicon.

The first applications of HTS in computers may be interconnects—electrical pathways joining otherwise conventional chips. Signal dispersion and other problems associated with transmitting electrical pulses within the processor limit performance; practical means for incorporating HTS interconnects should find ready application in large and powerful machines.

Moreover, at liquid nitrogen temperatures, superconductors and semiconductors could operate compatibly in hybrid designs. Ordinary semiconductors cannot be used at liquid helium temperatures; even if they could be made to operate in otherwise satisfactory fashion, semiconductors would dissipate too much heat, overwhelming the cooling system. Given that hybrid LTS-semiconductor systems are not feasible, past work on Josephson computing has involved either all-superconducting chips, or unique designs with controlled temperature gradients. The Hypres data sampler, for example, uses an integrated circuit cooled to liquid helium temperature on one end only—that end holding about 100 LTS JJs.

Three-terminal devices could be a big step forward in superconducting electronics, making possible logic designs at the chip level much like those now used with semiconductors. It could well be, however, that major advances in HTS electronics would come only with devices that departed in a major way from currently known electronic devices. The first requirement, in any case, is mastery of thin-film fabrication technology.

Military Systems

As table B-1 indicated, possible defense applications of superconductivity range from shielding against nuclear blasts to high-speed computers and motor-generators for ships. Conceptually, there may be little difference between military and commercial applications. But in practice, differences will be pervasive at levels all the way from devices and components (e.g., radiation hardening) to the system configuration itself (cost-performance tradeoffs much different than for commercial markets). Computing requirements for smart weapons—for example, real-time signal processing—tend to be quite different from those important in the civilian economy. Thus, as development proceeds, military uses of superconductivity will diverge in many respects from civilian applications.

Some of the military applications could be compelling. Submarine detection with SQUID-based sensors, for instance, offers at least a factor of 10 improvement over current methods. Conventional electric generators for shipboard or vehicle use, or for producing electric power under battlefield conditions, produce about 2 horsepower per pound; prototype LTS generators have already reached 25 horsepower per pound. Superconducting coil or rail guns promise increases in projectile velocities of 5 to 10 times.

The U.S. Department of Defense (DoD) has funded superconductivity R&D since the early 1950s, contributing to the development of large, high-field magnets, electrical machinery, LTS sensors, and superconducting computers. DoD (and the Department of Energy) also supported much of the materials processing R&D that proved necessary to achieve high current densities in LTS wire and cable. Since 1983, the R&D objectives of DoD programs in LTS have been redirected, and the programs have grown, as a result of the Strategic Defense Initiative (SDI).

For SDI, HTS shielding, waveguides, and sensors (for use in space) hold obvious attractions, while LTS work also continues; early in 1988, Bechtel and Ebasco began an SDI-funded design competition on LTS magnetic energy storage for powering ground-based free-electron lasers. SDI has also targeted very high-frequency communication systems, where LTS could offer substantial improvements in performance and extended frequency range. Here, the 1-2-3 ceramics seem to offer theoretically promising electronic characteristics (i.e., larger energy gaps). They would also avoid the many practical problems that liquid helium cooling poses in a military environment.

DoD has also renewed its attention to two of the prospective high-field, high-power applications—ship propulsion, and coil/rail guns. Military funding of R&D on LTS machinery began in the middle 1960s, with a 3000-horsepower prototype completed several years ago. Magnetohydrodynamic (MHD) thrusters offer a wholly different alternative, doing away with propellers, as well as shafts and gearing. In 1978, the Defense Advanced Research Projects Agency began funding R&D on electromagnetic launchers, or coil/rail guns. The initial goal, apparently, was a cannon for the Navy. With the advent of SDI, much of the DoD work has been redirected toward higher velocity systems, capable of launching a projectile into space. Like the commercial applications, the requirements, whether for machines or for coil/rail guns, start with good conductors.

Developing the Superconductivity Technology Base

Table B-3 gives a sampling of expert opinion on timing for a number of the applications discussed above. Without too much oversimplification, the R&D needed for supporting these and other applications can be pictured as in figure B-3.

Leaving aside military applications, particularly those in which the superconductor serves as a passive shielding medium, sensors and other relatively straightforward electronics applications will probably come first. As noted earlier, without new and much more tractable families of HTS materials, learning to make practical wire and cable will be a long and tedious process. As a result, the high-current, high-field applications will be slower in reaching the marketplace than thin- and thick-film electronics.

The R&D tasks outlined in figure B-3 will take a wide range of skills. Materials synthesis and characterization demands well-equipped laboratories and sophisticated experimental techniques—e.g., X-ray and neutron diffraction, electron microscopy, molecular beam epitaxy. Making thin and thick films of the 1-2-3 materials with adequate current-carrying capacity will probably mean oriented grain structures—a good deal more difficult in production than in the laboratory. Fabricating useful Josephson junctions will mean controlling the deposition of very thin layers. The processing techniques are likely to be more demanding than related semiconductor processing technologies.

Still, there is much that can be learned from related technologies, not only in microelectronics, but in ceramics. Applications of both structural and electronic ceramics demand very pure starting materials, careful control of processing (and thereby structure), and sensitive nondestructive inspection techniques. Some of this experience base will translate to HTS, especially to fabrication processes for filaments and wires.

As figure B-3 suggests, cryogenics technologies will be needed for most applications of HTS (in the absence of room-temperature superconductivity). Space is the exception. Even if much higher transition temperatures emerge, good performance may still require cooling—e.g., to minimize electrical noise, or increase current-carrying capacity. Although much of the speculation concerning HTS has assumed liquid nitrogen cooling, closed-cycle refrigeration systems can reach temperatures as low as 10 K, and would probably be the technology of choice in many systems.

Much of the R&D needed for commercialization of HTS will have to go on more-or-less simultaneously. For simplicity, one-way arrows join the boxes in figure B-3; a more realistic picture would be full of feedback loops representing the flows of knowledge accompanying development of a complex new technology (ch. 2). As conductor fabrication technology evolves, the design constraints for magnets and machines will take shape. System level studies of digital processors will feed back to the device level.

Developing the technology base in HTS means multidisciplinary research, and productive interactions among universities, national laboratories, and industry. Developing a technology base quickly, so that U.S. industry can keep up with Japanese industry, will mean taking risks, and managing overlapping R&D projects. The examples of industries ranging from automobiles to microelectronics (ch. 2) demonstrate that competing in HTS will require an R&D system that effectively supports parallel development on many different but inter-related problems.

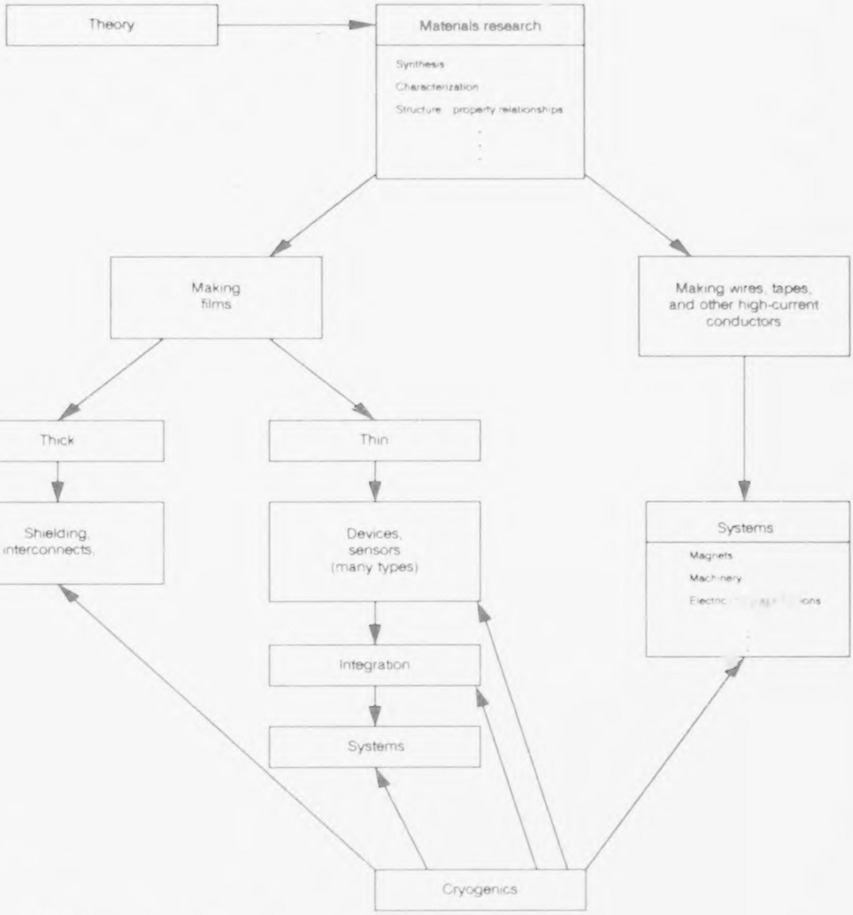
Table B-3.—Estimated Development Times for Prospective HTS Applications

Application	Time[a]
SQUIDs	Less than 5 years
Sensors	5 years
Computer interconnects	Less than 5 years
Superconducting computer	Long-term
Multifilamentary composite cable	5 to 10 years
Magnet system	over 10 years
Magnetic energy storage	Long-term
Transmission lines	Long-term
Electrical generators	Long-term

[a] Until small-scale commercial production.

SOURCE: "Technology of High Temperature Superconductivity," prepared for OTA by G.J. Smith II under contract No. 23-2100, January 1988, p. V-97, based on interviews.

Figure B-3. – HTS R&D


Theory
Materials research
Synthesis
Characterization
Structure - property relationships
Making films
Making wires, tapes, and other high-current conductors
Thick
Thin
Shielding, interconnects,
Devices, sensors (many types)
Systems
Magnets
Machinery
Electric utility applications
Integration
Systems
Cryogenics

SOURCE: Office of Technology Assessment, 1988

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FEDERAL RESEARCH PROGRAMS IN SUPERCONDUCTIVITY

COMPILED BY

FEDERAL COORDINATING COMMITTEE ON SCIENCE,
ENGINEERING AND TECHNOLOGY

COMMITTEE ON MATERIALS (COMAT)

SUBCOMMITTEE ON SUPERCONDUCTIVITY

MAY 1988

INTRODUCTION

With the recent discovery of new classes of materials which superconduct at temperatures attainable with liquid nitrogen, research activity in superconductivity throughout the Nation has increased. To assist coordination of the Federal programs, the Office of Science and Technology Policy's (OSTP) Committee on Materials (COMAT) established a Subcommittee on Superconductivity. One purpose of the subcommittee is to assist in the coordination of superconductivity programs in various Federal agencies through the exchange of information and discussion of problems of mutual interest. In this context the subcommittee has compiled information on funding and programs in superconductivity research, contained in this document.

To date, the subcommittee has met six times, the first meeting on May 29, 1987. The meetings have been held in the Washington area at various Federal agency locations. At two of the meetings, representatives from industry were invited to discuss their programs and plans with the subcommittee. One meeting was held at the National Institutes of Health where the subcommittee was able to discuss superconductivity applications in medical resonance imaging with experts. The other meetings have consisted primarily of information exchange on programs, plans, studies and meetings. Early in the subcommittee's existence, it became clear that a mechanism for keeping researchers and managers aware of basic research results in a timely fashion was essential. It was decided by the subcommittee to expand the already existing DOE Ames Newsletter, "High T_c Update," to cover all the agencies. This newsletter now receives support from other agencies (DARPA, AFOSR, ONR, NSF) in addition to DOE and has been able to expand its coverage to over 2,000 participants throughout the world.

The subcommittee has prepared this document to provide readers with an overview of the current Federal research programs in superconductivity. Each agency has prepared the description of their goals and programs included in this report. A subcommittee membership list is included, together with the Charter of the Subcommitee.

Louis C. Ianniello
Chairperson
Superconductivity Materials
Subcommittee of COMAT

SUMMARY

Total funding by agency for superconductivity research is contained in Table I for FY 1987, FY 1988 and FY 1989. Table II further estimates the funding for high temperature superconductivity and low temperature superconductivity. These funding levels for the programs described are estimated by each agency, since superconductivity research is often part of a larger program and not an agency budget line item. The figures represent the particular program manager's assessment of the amount of superconductivity work in the whole program to which it relates.

The Subcommittee noted that low temperature or conventional superconducting research and development utilizing the conductors such as niobium-titanium alloy or niobium-tin compound which require liquid helium as a coolant is important, and has to continue. Even though the new high temperature superconductors show much promise for efficient systems and possibly new applications, their development may be a long way off. In the meantime, concrete applications using existing materials are economically possible. Additionally, the two types of activities complement one another with respect to equipment and personnel.

Overall funding for superconductivity research is estimated to rise from $84.6 million in FY 1987 to $205.4 million in FY 1989, with high temperature superconductivity going from $44.9 million to $135.0 million, respectively.

Five agencies, Department of Defense (DOD), Department of Energy (DOE), National Science Foundation (NSF), Department of Commerce (NBS), and National Aeronautics and Space Administration (NASA), have substantial efforts underway in superconductivity with the first two the largest. The DOD estimates a rise in funding from $26.0 million to $71.0 million, FY 1987 to FY 1989, with the high temperature portion going from $19.0 million to $63.0 million, respectively. The Department of Energy estimates a rise in funding from $41.0 million to $94.8 million, FY 1987 to FY 1989, with the high temperature portion going from $12.5 million to $38.7 million, respectively.

The DOD has interests and research in superconductivity for applications which cover defense needs ranging from electronics to electromagnetic launchers and energy storage devices. Of all the agencies, the DOD has the most interest in electronic devices for applications such as signal processing, sensors, and antennas, for example. Although the DOD also has interests in electrical energy conversion, storage and distribution, the DOE program focuses on these latter areas and also large magnets for accelerators and fusion plasma containment. The NSF is funding universities to pursue basic research in superconductivity covering many disciplines. NBS has an interest in superconductivity for standards and instrumentation. The NASA program is aimed at applications special to their needs in aeronautics and space, especially for weight and volume savings.

From Tables I and II it is seen that the Federal funding has grown rapidly and the agencies have underway a significant effort in the field of superconductivity. The research on high temperature superconductors was initiated quickly in FY 1987 through redirections and is planned for a significant increase in FY 1988 and FY 1989. Much of the research is directed at overcoming the known technical obstacles of the present high temperature superconductors. Research is underway on bulk powder processing and thin film processing for the purpose of raising the critical current density. Basic research is being pursued to understand the structure and mechanism of superconductivity. Synthesis and measurements of new exploratory materials are also underway, as are studies of mechanical properties and thermal and chemical stability. In addition, assessments have been completed or are in process on the relative value of the new high temperature materials for various applications.

In reviewing the overall content of the United States superconductivity research activity, the COMAT Subcommittee identified the area of low-temperature superconducting niobium digital electronics as a very critical one and one which is not being addressed adequately in the United States. The speed and ultra-low power dissipation of superconducting microelectronic devices and circuits strongly suggest that they will ultimately outstrip semiconductor electronics in large-scale computing applications. Yet this area has lain virtually abandoned in the United States. The very compact size and very modest cooling requirements for superconducting computers will not only offer significant advances in typical ground-based applications, but will even permit their use in aircraft and spacecraft which could not accommodate a conventional Cray-type supercomputer with its very large power requirement and enormous roof-top cooling system. Development of low-temperature superconducting niobium electronics technology is highly desirable in its own right, because it offers potential for relatively near-term application. In addition, those nations which most aggressively advance low-temperature superconducting electronics will be in the best position to carry the superconducting electronics revolution to its ultimate conclusion by eventually executing in high-temperature superconducting materials the devices, circuits, and architecture which they earlier invented using low-temperature superconducting materials. The Subcommittee intends to discuss this problem in more detail in the future to recommend further coordination and other actions as appropriate.

Along with the adequate rise in planned funding for research, the Subcommittee believes the Federal effort has been well coordinated among the agencies and shared with the private sector. This coordination has taken place especially through activities of this Subcommittee, the High-T_c Update Newsletter, technical society meetings, publications, participation by agency representatives in various review meetings, the Federal Conference on Superconductivity held in July 1987, and various outreach activities such as the establishment of information centers, the holding of workshops and other meetings with participation of all sectors of the economy.

TABLE I

FEDERAL FUNDING[1/] LEVELS IN SUPERCONDUCTIVITY (MILLION DOLLARS)

	FY 1987	ESTIMATE FY 1988	ESTIMATE FY 1989
DOE	$ 41.0	$ 66.9	$ 94.8[2/]
DOD	26.0	53.0[3/]	71.0[2/]
NSF	13.7	17.5	20.1
DOC	1.1	2.8	9.3
NASA	2.6	6.9	9.8
DOI	0.1	0.1	0.1
DOT	0.1	0.3	0.3
TOTAL	$ 84.6	$ 147.5[3/]	$ 205.4[2/]

1/ HIGH TEMPERATURE AND LOW TEMPERATURE

2/ NOT INCLUDING $46.9 MILLION FOR THE PROCUREMENT OF SUPERCONDUCTING WIRE AND DESIGNED SYSTEMS.

3/ NOT INCLUDING $11 MILLION FOR PROCUREMENT OF DESIGNED SYSTEMS.

MAY 1988

TABLE II

FEDERAL FUNDING IN SUPERCONDUCTIVITY

(MILLION DOLLARS)

	FY 1987	ESTIMATE FY 1988	ESTIMATE FY 1989
HIGH TEMPERATURE			
NSF	11.7	14.5	17.2
DOE	12.5	27.2	38.7
DOD	19.0	46.0	63.0
DOC (NBS)	1.1	2.8	9.3
DOT	0	0	0
NASA	0.5	4.2	6.7
DOI	0.1	0.1	0.1
TOTAL HIGH TEMP.	44.9	94.8	135.0
LOW TEMPERATURE			
NSF	2.0	3.0	2.9
DOE	28.5	39.7	56.1[1]
DOD	7.0	7.0[2]	8.0[3]
DOC (NBS)	0	0	0
DOT	0.1	0.3	0.3
NASA	2.1	2.7	3.1
DOI	0	0	0
TOTAL LOW TEMP.	39.7	52.7	70.4
TOTAL	84.6	147.5	205.4

[1] NOT INCLUDING $33.9M FOR PROCUREMENT OF SUPERCONDUCTING WIRE.
[2] NOT INCLUDING $11.0M FOR MAGNETIC ENERGY STORAGE SYSTEM.
[3] NOT INCLUDING $13.0M FOR MAGNETIC ENERGY STORAGE SYSTEM.

MAY 1988

SUPERCONDUCTIVITY
IN THE
DEPARTMENT OF ENERGY

February 5, 1988

A report from the Subcommittee on Superconductivity of the Energy Materials Coordinating Committee

FOREWORD

This survey of the Department of Energy's activities in superconductivity was prepared by the Subcommittee on Superconductivity of the Energy Materials Coordinating Committee, also known as EMaCC. EMaCC is charged with the coordination of the Department's research and development activities in the area of materials. These activities cover fundamental and applied research and development. The amount spent by DOE in Fiscal Year 1986 on materials was about $448 million. Among these activities, a large effort in superconductivity always has existed in the Department and its predecessor agencies. For Fiscal Year 1987, the amount spent on superconductivity was $45 million. In Fiscal Year 1988, about $67 million is planned to be spent on superconductivity, not including the Small Business Innovation Research program for which estimate could not be made as of the date of this report. These amounts cover both conventional and high-temperature superconductivity. The Department is interested in superconductivity because superconductors can be used in accelerators for high energy and nuclear physics, for magnets to confine plasmas for fusion energy, to conserve electricity from generation, transmission, distribution and end use, and for a other variety of applications including those in the defense programs. To a large extent, the small superconducting industry in the United States owes its existence to the development and procurement of superconductors by the Office of High Energy Physics, starting with magnets for bubble chambers in 1969 and continuing today with magnets for the Superconducting Super Collider.

The discovery of the high-temperature superconductors which allow the use of liquid nitrogen rather than liquid helium could have a broad impact on the use of superconductors in many areas of interest to the Department. In many cases, such as the superconducting transmission line supported by Conservation and Renewable Energy, systems have already been demonstrated with low temperature superconductors. Use of liquid nitrogen rather than liquid helium could make many of these systems economically viable. Also, as with any new discovery, fundamental research, such as that supported by the Office of Basic Energy Sciences, may give rise to many applications yet unconceived.

In view of these many and varied interests and the significant increase in the level of effort in superconductivity, EMaCC formed the Subcommittee on Superconductivity in May 1987 to coordinate specifically the activities in superconductivity. This report briefly describes those activities. More detailed information on specific programs is available by calling the individuals whose names are given as contacts.

Iran L. Thomas, Director
Division of Materials Sciences
Office of Basic Energy Sciences

Chairman, Subcommittee on Superconductivity
Energy Materials Coordinating Committee

FY 1989 CONGRESSIONAL BUDGET
SUPERCONDUCTIVITY CROSSCUT
(Dollars in Thousands)

DECISION UNIT	FY 1987	FY 1988	FY 1989
HIGH TEMPERATURE RESEARCH			
Office of Energy Research			
Basic Energy Sciences	$10,200	$15,140	$16,670
High Energy Physics	200	200	300
Subtotal, Office of Energy Research	10,400	15,340	16,970
Defense Programs	1,630	6,667	6,667
Office of Fossil Energy			
Advanced Research and Technology Development - Materials	198	294	220
Office of Conservation and Renewable Energy			
Energy Utilization Research	100	425	2,000
Electric Energy Systems and Energy Storage	200	4,425	12,872
Subtotal, Office of Conservation and Renewable Resources	300	4,850	14,872
TOTAL, High Temperature	$12,528	$27,151	$38,729
LOW TEMPERATURE RESEARCH			
Office of Energy Research			
Basic Energy Sciences	$ 1,000	$ 1,000	$ 600
Magnetic Fusion Energy	10,700	10,100	8,500
High Energy Physics	16,000	28,130	46,650 *
Subtotal, Office of Energy Research	27,700	39,230	55,750
Bonneville Power Administration			
Electric Energy R&D	20	150	300
Office of Fossil Energy			
Magnetohydrodynamics	200	275	0
Office of Conservation and Renewable Energy			
Electric Energy Systems and Energy Storage	600	0	0
TOTAL, Low Temperature	28,520	39,655	56,050
TOTAL, Department of Energy	$41,048	$66,806	$94,779

*Not including procurement of $33.9 M for superconducting wire.

INTRODUCTION

The Department of Energy has had a long standing interest in superconductivity because of its use in high energy physics, fusion, and its potential use in a variety of other energy systems such as electrical transmission, generation, and storage. Since the days of the Atomic Energy Commission, a predecessor to the Department, support of superconductivity research and procurement of superconducting devices by the Department of Energy have had a major positive impact on the economic viability of the small U.S. superconductor industry. For example, the Department's Fermi National Accelerator Laboratory completed the world's largest superconducting project, the superconducting Tevatron accelerator, which contains 1,000 magnets using 60 tons of niobium-titanium superconductor supplied by industry. The Department's involvement in the development of superconductors has been and will continue to be substantial, as will our contributions to the development of a U.S. superconductor industry.

Because of its wide-ranging research responsibilities, the Department of Energy has traditionally had the largest Federal program in superconductivity. This program has ranged from basic research on theory and structure to the actual construction and use of large, superconducting magnets and of superconducting electrical generation, transmission, and storage devices. With the discovery this year of superconductors that can operate at temperatures attainable with liquid nitrogen (77 Kelvin) rather than the much colder liquid helium (4 Kelvin), DOE has been at the forefront of both the research and the technology transfer activities.

Unlike the niobium-titanium superconductor, the new materials are brittle, difficult to fabricate, and have some technical obstacles which must be overcome before any widespread applications are possible. DOE research has already made significant contributions in the areas of theory, structure determinations using major DOE facilities, processing the ceramics into wire and ribbon form, synthesis of materials, and understanding the origin of the unusual properties of these oxides.

The Department also is moving rapidly to couple the DOE program with U.S. industry and other researchers. The Department co-sponsored the White House Conference on Superconductivity held on July 28, 1987, at which President Reagan announced his Superconductivity Initiative. The Department has established three centers to conduct research on superconductivity and to act as sources of information in response to the President's Superconductivity Initiative. These centers are at Argonne National Laboratory, Lawrence Berkeley Laboratory, and Ames Laboratory. Argonne National Laboratory will be the focal point for information on conductor applications, such as magnets for accelerators and medical imaging devices, and conductors for electrical power transmission, storage, and conversion. Lawrence Berkeley Laboratory will serve as the focal point for applications in electronic devices, such as radiation detectors, magnetic field sensors, and computer applications. Ames Laboratory will coordinate information on the fundamental and generic properties of the high temperature superconductors.

Besides these centers, which are in operation now and will provide day-to-day information to U.S. industry, the DOE Office of Scientific and Technical Information has established a computer-based data center. This center will be a repository of reports and work in progress accessible though a computer link. In a closely related initiative, the Secretary of Energy has asked Los Alamos National Laboratory to lead an effort to examine mechanisms for cooperative ventures among DOE laboratories, U.S. industry and universities. Los Alamos National Laboratory will lead this study with the intent of extending the partnership mechanisms to other laboratories. For information regarding these activities at the respective institution, call (312) 972-7928 at Argonne National Laboratory, (415) 486-4755 at Lawrence Berkeley Laboratory, (515) 294-3877 at Ames Laboratory, (615) 576-1303 at the Office of Scientific and Technical Information, and (505) 667-3839 at Los Alamos National Laboratory. Support for superconductivity research is given in Table I.

Basic research is the responsibility of the Office of Basic Energy Sciences. Research and development pertinent to their respective programs is done in the Office of Fusion Energy, Office of High Energy Physics, Office of Conservation and Renewable Energy, Office of Fossil Energy, and the Defense Programs. The activities in superconductivity of each of the Offices of the Department are described in the following sections.

Table I - Funding for Superconductivity

	FY 1987		FY 1988 (estimate)	
	Conventional	High Temp.	Conventional	High Temp.
Office of Energy Research				
Basic Energy Sciences	1,000,000	10,200,000	1,000,000	15,140,000
Fusion Energy	10,700,000	0	10,100,000	0
High Energy and Nuclear Physics	16,000,000	200,000	28,130,000	200,000
Defense Programs	0	1,630,000	0	6,667,000
Office of Conservation and Renewable Energy				
Energy Utilization Research	0	100,000	0	425,000
Energy Storage and Distribution	600,000	200,000	0	4,425,000
Office of Fossil Energy				
Advanced Research and Tech. Development		198,000		294,000
Magnetohydrodynamics	200,000	0	275,000	0
	28,500,000	12,528,000	39,505,000	27,151,000

The Small Business Innovation Research program obligated $3,587,000 for awards for work on conventional superconductors in Fiscal Year 1987. An estimate of the Fiscal Year 1988 funding cannot be given because awards have not been made.

OFFICE OF ENERGY RESEARCH

The Office of Energy Research includes in its organization the major programs of Basic Energy Sciences, Fusion Energy, High Energy and Nuclear Physics, Health and Environmental Research, and manages the Small Business Innovation Research program for the Department. The Director of Energy Research also advises the Secretary of Energy on all of the Department's research programs. The first three of the major programs and the Small Business Innovation Research program have significant efforts in superconductivity which are described below.

Office of Basic Energy Sciences

Contacts: Advanced Energy Projects -- R. Gajewski, (301) 353-5995;

Chemical Sciences -- R. S. Marianelli, (301) 353-5804;

Engineering & Geosciences -- O. P. Manley (301) 353-5822;

Materials Sciences -- I. L. Thomas, (301) 353-3427.

The Office of Basic Energy Sciences conducts a program of basic energy-related research to provide the fundamental scientific foundation on which the Nation's future energy options depend. The subject matter of the research relates to the prudent use and the environmental suitability of the supply of energy. The product of the Office of Basic Energy Sciences is knowledge, insight, and information needed for the development of an optimized national energy system.

Superconductivity has always been a topic of research within the Office of Basic Energy Sciences and that of its predecessor organizations because of its potential applications in areas such as fusion energy and electrical transmission, generation, and storage. Since the discovery of the new class of high-temperature superconductors late last year, research within the Office of Basic Energy Sciences, in the Division of Materials Sciences, has been redirected to understand the behavior of these materials and to overcome the scientific and technical obstacles which must surmounted to use the new materials in practical systems.

Research underway now in the Division of Materials Sciences addresses: synthesis, understanding the crystalline structure, theory, physical and chemical properties, processing to make bulk materials such as wires or tapes, characterization of the bulk materials, and mechanical properties. This research program deals with fundamental questions regarding the mechanism by which these materials become superconducting, the fundamental parameters of critical temperature, critical magnetic field, and critical currents, and synthesis of new materials to expand the range of these parameters. The research program also addresses the processing techniques needed to make reliable forms, such as films, wires, and tapes, with optimum superconducting properties and to develop understanding of the mechanical properties of these materials. Two joint laboratory programs were established to make the best use of the strengths of the laboratories. Argonne National Laboratory, Ames Laboratory, and Brookhaven National Laboratory concentrate on processing. Oak Ridge National Laboratory, Los Alamos National Laboratory, and Lawrence Berkeley Laboratory concentrate on synthesis, thin films, and devices. Some work continues on low-temperature superconductors, such as the heavy-fermion and organic superconductors.

The research programs have already made substantial progress. Crystal and magnetic structures have been determined using neutron scattering and synchrotron-light scattering. Several different synthesis methods have been developed and used to prepare materials with different compositions. Theoretical calculations based on the crystal structure are beginning to shed light on the mechanism of superconductivity. Tapes and wires have been made. To help coordinate all this activity, a newsletter, "High Tc Update," has been established and is distributed to over 2,000 researchers. Four meetings to coordinate the DOE laboratory efforts have been held, one in January, another in February, the third in May, and the fourth in September 1987. A fifth meeting will be held in January 1988. All meetings except the first two have included representatives from industry and universities.

The Division of Chemical Sciences supports research on the synthesis of chemical precursors for the materials. Better precursors would provide better control of the mixtures of elements needed to make the materials. Advanced Energy Projects has started a project in Fiscal Year 1988 to develop a superconducting motor. The Division of Engineering and Geosciences is planning research on the engineering problems associated with the use of these materials in devices. These problems range from contacts to normal conductors to the design of hybrid microelectronic devices with superconducting interconnecting links. A workshop to identify the other related research opportunities in the Engineering Research program is planned for late Spring 1988.

Funding:

	Fiscal Year 1987	Fiscal Year 1988 (Estimate)
Materials Sciences	$11,200,000	$15,200,000
Chemical Sciences	----	500,000
Advanced Energy Projects	----	380,000
Engineering & Geosciences	----	60,000
Total	$11,200,000	$16,140,000

Office of Fusion Energy

Contact: D. S. Beard, (301) 353-4958

The goal of the magnetic fusion program is to establish the scientific and technological base required for fusion energy. The specific objectives of the program are to predict the behavior of plasma confined in fusion-relevant confinement systems that could develop into reactors, to develop unique fusion components that can operate under conditions required for fusion energy sources, to provide options for private sector investment and commercial development of fusion, and to foster industrial experience with the unique aspects of fusion science and technology.

The interest in superconductivity in the Office of Fusion Energy stems from the need for large magnets needed to confine the plasma in fusion reactors. Magnet systems have been made from low-temperature superconductors. The work continues now directed at development of large, 10 to 15 Tesla, steady state and pulsed superconducting magnets using low-temperature, niobium-tin superconductors. The low-temperature structural and electrical materials needed to fabricate these large magnets are an integral part of the program. The four elements of the program are Base Technology, Large Coil Program, High Field Magnets, and Pulsed Magnets.

Base Technology supports research on improved superconducting materials and processes, low-temperature magnet structural materials, and powder metallurgical processes for advanced conductors, and innovative magnet systems concepts, including magnetic limiters and divertors. The Large Coil program consists of an international, collaborative testing of 6 large, 8 Tesla, toroidal field coils. The test will determine the large-scale effects of electromagnetic forces, heating, and transients under simulated tokamak conditions. The large coil tests will be completed in Fiscal Year 1988 and emphasis will shift to high-field, steady-state and pulsed magnets. The High Field Magnet program consists of the development of high-field magnets between 12 to 15 Tesla. The development includes superconductor and coil design, fabrication and testing to support tokamak design studies. In the Pulsed Magnet program, a 10 Tesla magnet capable of being pulsed at 1 Tesla per second will be built and tested.

In the area of high temperature superconductors, the Office of Fusion Energy plans to study the impact on fusion reactor designs and economics and to maintain awareness of technical progress.

Funding:	Fiscal Year 1987	Fiscal Year 1988 (Estimate)
Base Technology	$1,300,000	$2,200,000
Large Coil Program	7,500,000	1,000,000
High Field Magnets	1,200,000	4,700,000
Pulsed Magnets	700,000	2,200,000
Total	$10,700,000	$10,100,000

Office of High Energy and Nuclear Physics

Contact: High Energy Physics D. F. Sutter, (301) 353 5228

Nuclear Physics C. R. Richardson, (301) 353 3613

This Office conducts a basic research program in two main areas, High Energy Physics and Nuclear Physics. Understanding the fundamental nature of matter and energy and their transformation is the mission of High Energy Physics. The Department of Energy provides about 90 percent of the Federal support for this program; the National Science Foundation provides the remainder. The Nuclear Physics program is the major Federal research effort concerned with advanced experimental and theoretical studies of the interactions, structure, and other fundamental characteristics of nuclei. Both of these programs operate major accelerator facilities. An ancillary goal of High Energy and Nuclear Physics is the development of certain advanced technologies based on superconductivity because continuing improvements in this technology will lead to higher particle energies, more intense particle beams, and greater particle detection sensitivity. For these reasons, the Office of High Energy and Nuclear Physics has supported much of the development of practical superconductors in the United States. Over the past 2 decades the accumulated funding for research and development in superconductivity by the Office of High Energy and Nuclear Physics is well over $100 million. Purchases of superconducting components have been about $200 million.

The Division of High Energy Physics directly supports the development of the conductor technology needed to advance the performance of accelerators and detectors used in experimental high energy physics. Without superconductors, modern accelerators for high energy physics experiments could not be built because the operating power would be to expensive and the necessary magnetic fields could not be achieved. The first large scale use of superconductors, for the magnet in the 12 foot bubble chamber at Argonne National Laboratory was completed in 1969. The highest energy particle accelerator and collider in the world, the Tevatron at Fermilab uses 60 tons of niobium-titanium superconductor in about 1,000 magnets. A detector for this collider uses a large superconducting solenoid which is 3 meters in diameter and 5 meters long. Research and development on conventional, low temperature superconductors includes a large effort on the dipole magnets for the Superconducting Super Collider, special focusing magnets for the Stanford Linear Collider, solenoids, large detectors, and a generic program to improve the characteristics of niobium-titanium superconductor materials.

The Division of Nuclear Physics supports development of magnets for a Relativistic Heavy Ion Accelerator and work on superconducting radiofrequency cavities. The Continuous Electron Beam Accelerator Facility will use superconducting radiofrequency cavities that operate at about 2 K and should allow the power requirements for acceleration of the beam to be about 100 times less than with normal conductors. Another facility using superconductors is the ATLAS heavy ion research facility at Argonne National Laboratory, which uses superconducting radiofrequency cavities for its linear accelerator.

Although the Office of High Energy and Nuclear Physics supports some long range research and development on superconductivity, the major expenditures have been for engineering development and practical applications. To date, the only work on the high-temperature superconductors is a project with the specific goal of achieving high critical currents. Until high-temperature superconductors achieve higher current densities and show promise of meeting other requirements essential for high energy physics or nuclear physics applications, this Office does not plan extensive work on these materials.

Funding:

	Fiscal Year 1987	Fiscal Year 1988 (Estimate)
Magnet Development	$12,000,000	$18,130,000
Superconducting Materials	3,000,000	3,000,000
Radiofrequency Cavities	1,000,000	7,000,000
High Temperature Superconductors	200,000	200,000
Total	$16,200,000	$28,330,000

Small Business Innovation Research Program

Contact: S. J. Barish, (301) 353-3054

The Small Business Innovation Research (SBIR) program was mandated by the Small Business Innovation development Act of 1982 (Public Law 97-219, as amended). The SBIR program of the Department of Energy is administered through the Office of Energy Research. Although projects are supported from all technical areas of the Department save the Defense programs. The principal purposes of the SBIR program are to stimulate technological innovation, to use small business to meet Federal research and development needs, to foster and encourage participation by minority and disadvantaged persons in technological innovation, and to increase private sector commercialization derived from Federal research and development.

There are three phases to an SBIR project, of which the first two are supported by SBIR funds. Phase I awards evaluate the scientific or technical merit and feasibility of ideas proposed for investigation. The awards are for up to $50,000 for 6 1/2 months. Phase II is the principal research or research and development effort. The awards are for as much as $500,000 for up to 2 years. Phase III is the commercial application of the research or research and development by small business with non-Federal capital or, alternatively, with non-SBIR Federal contracts for products or services desired by the Government.

In Fiscal Year 1987, there were 11 Phase I and 7 Phase II awards made for projects in areas related to superconductivity. These projects included development of improved niobium-titanium, niobium-tin, and niobium-nitride superconductors, insulators, and cryogenic systems. All of these projects dealt with low-temperature superconductors for high-energy physics and fusion energy applications. The Fiscal Year 1988 solicitation includes a specific technical topic on the high-temperature superconductors. Many proposals are expected to be submitted under this topic. Superconductivity is included also in several other topics such as those relating to materials, fusion, and high-energy physics.

Funding:

	Fiscal Year 1987	Fiscal Year 1988* (Estimate)
SBIR	$3,587,050	----

* An estimate of the Fiscal Year 1988 funding cannot be given because awards have not been made.

DEFENSE PROGRAMS

Contact: A. E. Evans, (301) 353-3098

Defense Programs is responsible for the Department of Energy's programs for nuclear weapons research, development, testing, production, and surveillance; laser, heavy ion, and particle beam fusion; safeguards and security; international security; and the intelligence functions and classification of information. In addition, Defense Programs is responsible for the Nuclear Materials Production and Defense Waste and Transportation Management.

Within Defense Programs in the Office of Military Applications, the Office of Weapons Research, Development, and Testing (OWRD&T) directs research, development, and testing for the Nation's nuclear weapons program. Elements of OWRD&T with an interest in superconductivity are the Nuclear Directed-Energy Weapons Program for the Strategic Defense Initiative, the Inertial Fusion Program, and the Nuclear Weapons Technology program. OWRD&T maintains a broad technology base program in support of nuclear weapons development and other national security interests at Los Alamos National Laboratory (LANL), Lawrence Livermore National Laboratory (LLNL), and Sandia National Laboratories (SNL). Superconductivity in particular is an integral part of that technology base because of the potential near-term applications to national security programs.

The goal of the work in high-temperature superconductivity is to develop processes required to fabricate devices that may be useful in nuclear weapon systems that will lead to improved safety and reliability. Work includes thin film and bulk ceramics for infrared sensors, radio-frequency cavities, magnetic shielding, switches, bearings and solenoids. Supporting research for these activities involves characterization, studies of the effects of pressure and temperature, and optimization of processing parameters.

Funding:

	Fiscal Year 1987	Fiscal Year 1988 (Estimate)
Office of Military Applications	$1,080,000	$3,500,000
Institutional Supporting Research and Development at LANL, LLNL, and SNL	550,000	3,167,000
Total	$1,630,000	$6,667,000

CONSERVATION AND RENEWABLE ENERGY

Conservation and Renewable Energy formulates and directs programs for long-term, high-risk research and development for renewable and conservation technologies. These programs provide a general technology base which the private sector will be able to use to develop advanced renewable and conservation applications. Within Conservation and Renewable Energy, the Office of Energy Utilization Research and the Office of Energy Storage and Distribution have efforts in the area of superconductivity.

Office of Energy Utilization Research

Contact: J. J. Eberhardt, (202) 586-5377

The Office of Energy Utilization Research supports research aimed at understanding processes that underlie a variety of energy systems and end-use applications. Among the missions of this Office, energy efficiency is the principal focus of the Energy Conversion and Utilization Technologies (ECUT) Program. In Fiscal Year 1987, a preliminary assessment of the energy conservation potential of the new high temperature superconductors was made. In Fiscal Year 1988, detailed technical assessment will be made of the potential benefits to nine specific end-use areas: motors, transportation, power electronics, heat pumping, lighting, electromagnetic pumping, materials production, materials fabrication, and waste separations. This assessment is funded jointly through Conservation and Renewable Energy ($425,000), Fossil Energy ($75,000), and the Electric Power Research Institute ($500,000). The assessment will be completed in Fiscal Year 1988; and based on the conclusions of the assessment, an experimental program will be planned. A Program Opportunity Notice is expected to be published in the latter part of Fiscal Year 1988.

Office of Energy Storage and Distribution

Contact: K. W. Klein, (202) 586-2826

The Office of Energy Storage and Distribution is responsible for the development of systems that will help the private sector in developing reliable, low-cost, safe, and environmentally acceptable energy storage and distribution systems. Work in power delivery technologies includes superconducting transmission cables, electrical energy storage systems, generators, motors, transformers, and control and protective devices.

The Office of Energy Storage and Distribution (OESD) has carried out research on applying low-temperature superconductivity to electric power devices such as generators, ac transmission lines, and magnetic energy storage. The research covered a broad spectrum of the R&D cycle from research through technology demonstration. These activities, carried out from 1972 to 1986, have resolved many of the technical and economic issues related to the use of superconducting designs by the electric power industry. Market conditions and the lack of economic competitiveness with more mature technologies prevented the active introduction of these devices into the nation's electric network. The new higher-temperature superconducting materials offer a unique opportunity to enter these, and other applications, into the marketplace at competitive life cycle costs. OESD will continue to be responsible for all potential applications of superconductivity in the U.S. electric network.

The goal of OESD is to develop the practical application of high-temperature superconducting materials in electric power system equipment to reduce losses, improve performance and lower costs. It is recognized, however, that the present material does not satisfy any set of engineering requirements which would allow their use in cables, motors, generators or magnetic energy storage. In particular, the projected current at 77 K in expected magnetic fields is low. Also the bulk polycrystalline material is brittle, unstable, and exhibits grain boundary interference to current flow.

Early research on the low-temperature superconducting transmission system focused on developing a cable conductor and insulating materials capable of operating between the low temperatures and ambient without suffering mechanical damage. A fully synthetic polymer insulating tape was developed. This tape did not dissolve in dielectric fluids used in conventional underground cables. In FY 1987, $600,000 was allocated to Brookhaven National Laboratory to develop these synthetic insulation materials for ambient temperature cables.

In FY 1987, $200,000 was allocated out of the program for economic assessments and high temperature superconducting material development. During FY 1987, OESD carried out a preliminary assessment of the economic impact of using high-temperature (liquid nitrogen cooled) superconductors in systems previously designed for low temperature superconductors and established that both energy savings and cost savings could be substantial. This study is presented in the Argonne National Laboratory report, Advances in Applied Superconductivity: Goals and Impacts, A Preliminary Evaluation. For the purposes of calculating the potential economic benefit of the new material the assessment investigated the impact of lower cooling requirements and assumed that devices using the new material would otherwise perform as with the low temperature superconductors. This assumption was required since the capability for carrying current needed in these applications has not been demonstrated in conductors made of the new material.

For FY 1988, $4.425 million has been allocated for research needed to develop prototype equipment using advanced superconductors. The initial emphasis is on development of practical conductors which can carry the required current loads as well as exhibiting the strength, ductility and stability needed. A parallel effort will be carried out on the technology of devices using advanced superconductors in which the importance of conductor properties is examined. The device technology activities will also focus the conductor development program by establishing goals for properties of the superconducting material. This interaction will continue until device prototypes can be built and verification made of predicted performance. FY 1988 activities initiate this process which could result in early use of the new superconductors in electric power system equipment.

OESD will enlist laboratory involvement in practical conductor development and device technology development. This work will provide guidance to the direction of research needed to improve the material characteristics. Any intrinsic limitations on use of the high-temperature material in electric power systems will be identified. Any form of the material (i.e., film, monolith, composite) will be considered.

Device design approaches which are able to use known or anticipated characteristics of conductors using the ceramic materials are desired since it is anticipated that previous designs based on metallic conductors or superconductors will not be appropriate.

Technology transfer activities will be included at all stages of development and the identification of potential industry and academic team members will be made.

Funding:

	Fiscal Year 1987	Fiscal Year 1988 (Estimate)
Energy Conservation and Utilization Technologies	$ 100,000	$ 425,000
Energy Storage and Distribution	800,000	4,425,000
Total	$ 900,000	$4,850,000

FOSSIL ENERGY

Contact: F. M. Glaser, (301) 353-2786

The mission of Fossil Energy is to develop technologies that will increase domestic production of oil and gas and that will permit the nation to shift from less-abundant fuels to coal. Superconducting materials, particularly high-transition temperature materials, have considerable potential to improve a variety of fossil energy systems including high-field strength magnets for coal beneficiation and for magnetohydrodynamic systems.

The primary purpose of the Advanced Research and Technology Development program of Fossil Energy on superconducting materials is to conduct coordinated research and development on ceramic superconducting materials with major emphasis on the sol-gel synthesis and microwave sintering of these new ceramics. Tape and wire configuration will be made in structures including normal conductors and monolithic ceramic shapes. This work started in Fiscal Year 1987 at Oak Ridge National Laboratory and at Idaho National Engineering Laboratory. Besides the experimental work, Fossil Energy contributes to the joint assessment between the Department of Energy and the Electric Power Research Institute of applications of high-temperature superconductors in energy systems. This assessment will be completed in Fiscal Year 1988. In the Magnetohydrodynamics program, elements of internally cooled, cable superconductors, made of conventional superconductors, will be designed, built, and tested.

Funding:

	Fiscal Year 1987	Fiscal Year 1988 (Estimate)
Advanced Research and Technology Development	$198,000	$294,000
Magnetohydrodynamics	200,000	275,000
Total	$398,000	$569,000

INTRODUCTION TO DEPARTMENT OF DEFENSE SUPERCONDUCTIVITY PROGRAMS

Nearly forty years ago, DoD was the first among U.S. agencies to fund low temperature research on a broad scale, both in in-house laboratories and through contract research programs. By providing helium liquefiers for a multitude of universities, and by providing related research funding, DoD pursued a vigorous and productive program in superconductivity. Today most well experienced U.S. superconductivity researchers have either received DoD superconductivity research support, or are second or third generation students of professors who did.

Early DoD superconductivity efforts addressed superconducting bolometers for the detection of infrared radiation, primitive superconducting digital computer switches, and magnetically suspended superconductor gyroscopes. In the basic science arena, F. London's classic works on superfluidity and on the macroscopic theory of superconductivity were published under the aegis of DoD. Shortly thereafter DoD-funded researchers made fundamental contributions on the isotopic dependence of the superconducting transition temperature, and this provided a firm experimental basis for development of the microscopic theory of superconductivity. In the late 1950's, DoD-funded research on the microscopic theory of superconductivity led to the celebrated Bardeen-Cooper-Schrieffer theory, for which the authors were awarded the 1972 Nobel Prize in physics. Also, over the years DoD funded researchers who made significant contributions to the steady advance of the highest temperatures at which superconductivity could be observed.

In the early 1960s, DoD-funded researchers made essential contributions to the science of the Josephson junction (JJ) and to the understanding of high magnetic-field, high-current-density superconductivity. The JJ studies and other superconducting device studies made possible magnetic and electromagnetic sensors of unprecedented sensitivity. The military potential of such sensors has been explored by DoD for magnetic detection of submarines and for the detection of weak electromagnetic signals from extremely low frequencies (ELF) through radio frequencies (RF) to the microwave region. There have been significant related technology-transfers to the civil sector in magnetocardiography, in magnetoencephalography, in magnetic geological prospecting, and in detectors for radio astronomy.

In the area of superconducting electronics DoD programs have made significant progress in both digital and analog superconducting electronic circuits, achieving the highest-quality transmission lines and the highest-speed electronic signal processors. In a joint DoD-IBM program very significant progress was made toward development of a very compact but very high-speed computer. Subsequent DoD contracts with a small firm, Hypres, Inc., demonstrated ultra-high-speed superconducting electronics based on high-performance more-stable materials. A spin-off of this activity is the world's fastest sampling oscilloscope, now available commercially as a diagnostic tool for development of high-speed semiconducting circuits, most of which will ultimately find use in military systems.

DoD has also been at the forefront in a number of aspects of large scale superconductivity technology. The pioneering superconducting cavity resonator particle accelerator at Stanford University was developed under a Navy contract. Moreover, on a small test bed ship the Navy has demonstrated the feasibility of superconducting generators and motors as replacements for heavy and cumbersome reduction gears as a means for transferring the high RPM power from a shipboard gas turbine to the low RPM power needed to drive the ships's propellor. In an Air Force program a very lightweight, ultra high power density superconducting generator was developed and tested as an approach to provision of electrical power for airborne directed energy weapons. In another large scale military application the very high magnetic fields required for gyrotron millimeter wave generating tubes were provided by superconducting magnets. Such tubes are essential for advanced microwave and millimeter wave surveillance, guidance, and communications systems. Parallel commercial large scale superconductivity efforts have led to use of superconducting magnets in the now-ubiquitous magnetic resonance medical imaging systems and in prototype electric generators suitable for public power grid applications. All of these civil applications can trace their origins in one way or another to early insightful DoD investments in superconductivity R&D.

Cited above are just a few examples of the very vigorous DoD efforts to provide, by means of superconductors, military capabilities which cannot be achieved by conventional means. The fact that superconductivity has not to date achieved widespread use in operational military systems is attributable primarily to the degree of refrigeration which has been required heretofore. In an effort to mitigate this obstacle, DoD has actively pursued the development of efficient refrigeration systems. In one very elegant example, DoD funded development of a miniature refrigerator, for which the heat exchanger, expansion orifice, and cold region are all contained in a thumbnail-size chip. Although this micro-miniature refrigerator has not yet achieved the very low temperatures required for the earlier generation of superconducting devices it is finding use for cooling semiconductor infrared sensors in missile guidance systems and for cooling the highest speed semiconductor chips. Now that the era of HTS has arrived, this remarkable little refrigerator will find widespread use with superconducting sensors and electronics. Indeed, successful tests with HTS materials have already been carried out.

On a broader scale, the era of HTS offers the potential that the myriad superconducting military applications, pursued so assiduously by DoD over the past forty-odd years will finally come to fruition. With a vast reservoir of knowledge and experience in superconductivity DoD is well poised to exploit the new-found promise of the novel HTS materials.

DoD SUPERCONDUCTIVITY BUDGET ESTIMATES

(These are working figures only and are subject to change.)
Prepared by T.G. Berlincourt, 697-3228, 4-1-88

I. Low-Temperature Superconductivity (LTS)

	($ Millions)		
	Actual FY 1987	Est. FY 1988	Est. FY 1989
Army	0	0	0
Navy theory, magnetic sensors for ASW and mine countermeasures, EM/IR sensors, superconducting electronics (signal processors, logic, memory), electric ship drive	4	4	5
Air Force occurrence, radar signal processing, inertial sensors, compact airborne generators	1	1	1
DARPA	0	0	0
SDIO EM/IR sensors, cavity accelerators, electromagnetic launchers, superconducting magnetic energy storage (SMES)	2	2*	2*
TOTAL	7	7	8

*Includes $11M in FY88 and $13M in FY89 for an engineering design competition for a large superconducting magnetic energy storage system.

II. High-Temperature Superconductivity (HTS)

	($Millions)		
	Actual FY 1987	Est. FY 1988	Est. FY 1989
Army theory, materials characterization, processing; applications to magnetic/EM/IR sensors, electronic signal processors, mine countermeasures, EM launchers	1	2	3
Navy occurrence, theory, materials characterization, processing, environmental stability; applications to magnetic/EM/IR sensors, electronics (signal processors, logic, memory), surveillance, communications, antennas, ASW systems, mine countermeasures, mm-wave generators, electric ship drive	5	7	9
Air Force fundamental mechanisms, occurrence, characterization, film processing; applications to terahertz radar signal processing, antennas, sensors, communications, navigation, pulsed power, directed energy weapons, airborne electric generators	4	7	8
DARPA synthesis, processing, fabrication; applications to motors, generators, EM launchers, pulsed power, sensors, interconnects, signal processors, A/D converters, antennas	4**	18	20
SDIO photon sensor/seeker focal plane arrays, signal processing, microwave and millimeter wave systems, antennas, magnetic and EM shielding, RF cavities, radar, power conductors, switches, fuzes, crowbars, energy storage, pulsed power transformers, flux traps	5	12	23
TOTAL	19	46	63
GRAND TOTAL (LTS + HTS)	26	53	71

**Includes $2M from the Balanced Technology Initiative

U.S. ARMY COOPERATIVE ON HIGH TEMPERATURE SUPERCONDUCTIVITY

1. Agency/Office: US Army Cooperative

2. Contact: Dr. Gerald J. Iafrate, ETDL, Ft. Monmouth, NJ, 201-544-4308/AV 995-4308 (datafax 201-544-4306)

3. Mission: The mission of the cooperative is to accurately access the state-of-the-art as it progresses while at the same time making significant device application contributions of our own at the 6.1 - 6.2 level. At the same time, the cooperative will conduct evaluations of potential high impact component and systems applications.

4. R&D Objective:

   The cooperative participants identified specific superconductivity-related technological capabilities which, within their mission objective, could provide the Army with much improved battlefield advantages. These technologies are ultrastable, low-noise, frequency control cavities for secure communication; magnetic confinement and shielding for light-weight radars; sensors and detectors for mm-wave and IR electronics; ultrahigh-energy pulse power conditioning for EM hyperkinetic weapons; hybrid semiconductor-superconductor devices, and interconnects and striplines for MMIC and VHSIC technologies.

5. FY 1987 Activities and Funding: Start-up phase; 300K.

6. FY 1988 Plans and Estimated Funding:

Technical Approach:

The in-house Army cooperative program has three components, all working in synergism to accomplish the specified component objectives: the synthesis, processing, and evaluation of thin film and bulk material; the development of an understanding of the microscopic mechanism responsible for high temperature superconductivity to improve material quality and to find man-made, externally controllable new devices; the design and implementation of specific devices to retrofit into existing functional components to achieve significant increase in performance of current systems.

The specific technical program tasks are as follows:

Materials: Synthesis - the development of high quality single crystal (RE)Ba_2 Cu_3O_{9-x} is imperative if the high temperature superconducting material is to achieve critical current densities high enough (10^5 amps/cm^2)

to be practical for technical applications. In the thin film area, epitaxial methods (both MOCVD and MBE) and sputtering are to be used to synthesize high quality high temperature superconducting material; a search is to be made for suitable growth methods for bulk single crystal materials based on Czochralski growth process.

Evaluation - High temperature superconducting materials are to be characterized for transition temperature (T_c), current density (J_c), magnetic field properties such as critical fields (Hc_1, Hc_2), and Meissner effect; in addition, we intend to use microwave absorption and point contact spectroscopy to determine the superconducting energy gap, x-ray crystallography of high temperature superconducting single crystal material, optical evaluation including photoluminescence, Raman and Brillouin scattering, and specific heat measurements. Material issues concerning environmental stability, mechanical strength, producibility in bulk, ribbon, or wire form, and brittleness will be addressed.

Device Applications: The near term objectives for device development include fabrication and testing of inductor and high-Q cavities as high precision tuning elements; design, fabrication, and testing of TWT magnetic circuits using high temperature superconducting cladding material to prevent flux leakage and greatly improve tube performance; producing ultrasensitive mm-wave and IR sensors, high temperature superconducting portable generators, and ultrahigh-energy pulse-power switches with virtually no dissipation at high repetition-rates.

Basic Concepts: The overall objective is to provide a better understanding of the basic mechanism and fundamental limitations of high temperature superconductivity and to integrate findings into device applications. The specific plan is to establish the limitations imposed by the fundamental atomic nature of high temperature superconductivity; examine exciton mechanism first proposed by Bardeen, Allender, and Bray to explain the occurrence of high temperature superconductivity; test exciton mechanisms using photoluminescence information obtained in the material evaluation phase; explore possibilities of creating exciton mediated high temperature superconductivity in $(RE)Ba_2Cu_3O_{9-x}$ intra/inter-material superlattices as well as in semiconductor superlattices with optically induced, exciton-mediated electron-electron attraction; couple basic concept efforts with material evaluation and device applications.

Resource Strategy:

The present strategy is to seed a small Army investment within the current in-house program, while deriving maximum leverage from external cooperative agreements through DARPA and SDI to further developmental opportunities.

Internal resources committed and technical responsibilities planned for members of the cooperative in FY88 to seed the in-house Army program are as follows:

LABCOM - ETDL: (350K) IR sensors, high-Q cavities, microelectronic, magnetic and pulse-power device applications, materials, electrical and optical characterization, and basic mechanisms and limitations at the atomic and molecular level of understanding.

MTL: (300K) materials synthesis and characterization, processing, x-ray crystallography, material stability, multicycle capability, producibility, and formability.

HDL: (300K) radiation susceptibility, microwave and IR properties, and basic mechanisms.

CECOM - CNVEO: (100K) IR sensors and characterization.

ARDEC - Benet Lab: (300K) hyperkinetic weapon components, characterization, and evaluation.

MICOM - No active program and no plans in immediate future; observer status.

AVSCOM - No active program and no plans in immediate future; observer status.

FY 88 AIR FORCE PROGRAMS IN SUPERCONDUCTIVITY

PRESENTED TO COMAT SUBCOMMITTEE
ON SUPERCONDUCTIVITY

22 JANUARY 1988

BY
DR. HAROLD WEINSTOCK
PROGRAM MANAGER FOR SUPERCONDUCTIVITY
AIR FORCE OFFICE OF SCIENTIFIC RESEARCH
AFOSR/NE
BOLLING AFB, DC 20332-6448

FY 88 AIR FORCE FUNDING OF SUPERCONDUCTIVITY

AFOSR CORE PROGRAM		
ELEC. & MAT'L SCIENCES	$2.0M	
CHEMISTRY	$ .4M	
TOTAL AFOSR CORE		$2.4M
SUPPLEMENT FOR AIR FORCE SUPER. CONSORTIUM		
BASIC RESEARCH (6.1)	$1.6M	
APPL. RESEARCH (6.2)	$1.5M	
TOTAL CONSORTIUM		$3.1M
AIR FORCE LABORATORIES		$2.0M
TOTAL AIR FORCE FUNDING		$7.5M
MANAGEMENT OF SDIO PROGRAMS IN ELECTRONIC SENSING AND SIGNAL PROCESSING		($4.0M)

FY 87 AIR FORCE FUNDING OF SUPERCONDUCTIVITY

AFOSR CORE PROGRAM		
ELEC. & MAT'L SCIENCES	$3.0M	
CHEMISTRY	$ .4M	
TOTAL AFOSR CORE		$3.4M
AIR FORCE LABORATORIES		$1.3M
TOTAL AIR FORCE FUNDING		$4.7M
MANAGEMENT OF SDIO PROGRAMS IN ELECTRONIC SENSING AND SIGNAL PROCESSING		($2.0M)

MAJOR GOALS OF AFOSR PROGRAMS IN SUPERCONDUCTIVITY

1. UNDERSTAND THE NATURE OF SUPERCONDUCTIVITY IN CERAMIC OXIDE MATERIALS
2. SEEK STABLE FORMS OF EVEN HIGHER TEMPERATURE SUPERCONDUCTORS
3. IMPROVE THE CURRENT-CARRYING CAPACITY AND STABILITY OF THIN FILMS OF THESE RELATIVELY HIGH TRANSITION TEMPERATURE (T_C) SUPERCONDUCTORS
4. FABRICATE, AND EVALUATE THE PHYSICAL AND ELECTRONIC PROPERTIES OF ALL-CERAMIC OXIDE JOSEPHSON JUNCTIONS
5. INVESTIGATE THE FEASIBILITY AND FUNDAMENTAL LIMITS OF NEW ELECTRONIC DEVICE ARCHITECTURES USING CONVENTIONAL AND HIGH T_C SUPERCONDUCTORS

AIR FORCE
SUPERCONDUCTING ELECTRONICS RESEARCH CONSORTIUM

GOALS

1. DETERMINE CONCLUSIVELY THE PHYSICAL MECHANISM(S) RESPONSIBLE FOR THIS NEW FORM OF SUPERCONDUCTIVITY

2. MAINTAIN AN EFFORT AND MONITOR RESEARCH ACTIVITY ELSEWHERE TO DISCOVER AND UTILIZE SUPERCONDUCTIVITY AT STILL HIGHER TEMPERATURES-AN ACTIVITY WHICH SHOULD BECOME EASIER ONCE THERE IS BETTER UNDERSTANDING OF THE UNDERLYING PRINCIPLE(S)

3. CHARACTERIZE AND OPTIMIZE THE QUALITY OF THIN FILMS SUITABLE FOR APPLICATION TO HIGH FREQUENCY SENSING AND HIGH SPEED SIGNAL PROCESSING

4. EXAMINE THE POTENTIAL FOR AND DEVELOP PROTOTYPES OF NEW CIRCUIT ARCHITECTURES AND DEVICES WHICH UTILIZE FILMS OF THESE MATERIALS EITHER EXCLUSIVELY OR IN CONCERN WITH COOLED SEMICONDUCTORS AND NORMAL METALS

5. PRODUCE A SUCCESSFUL TEST DEMONSTRATION OF ONE OR MORE OF THE PROTOTYPE DEVICES (E.G., AN ARRAY OF JOSEPHSON JUNCTION SENSORS OR SQUID-BASED INERTIAL GUIDANCE UNITS AT 77K)

INSTITUTIONS FUNDED BY AFOSR
IN SUPERCONDUCTIVITY

ELECTRONIC AND MATERIAL SCIENCES

- Cornell University
- Hypres
- Lincoln Laboratory
- MIT
- University of Minnesota
- Northwestern University
- Princeton University
- Rochester University
- Stanford University
- SUNY - Stony Brook
- University of Texas
- University of Utah
- Vanderbilt University
- University of Virginia
- Westinghouse Electric Corp.
- University of Wisconsin - Madison
- University of Wisconsin - Milwaukee

CHEMISTRY

- University of Arizona
- University of California - Los Angeles
- Clarkson University
- University of Florida
- MIT
- Universal Energy Systems
- University of Washington

AIR FORCE INTERESTS IN HTSC

RADC
- RADIATION EFFECTS
- SC GROUND PLANES IN MSW DEVICES
- MICROWAVE SURFACE IMPEDANCE (PART OF MIT/LL/RADC JOINT EFFORT)

AERO PROPULSION LAB (AFWAL/PO)
- SPACE POWER BUS
- KLYSTRODE ANE TWT MAGNETS

GEOPHYSICAL LAB (AFGL)
- CONTRACTING FOR IGU DEVELOPMENT

AVIONICS LAB (AFWAL/AA)
- ANALYSES OF ELECTRONIC APPLICATIONS

MATERIALS LAB (AFWAL/ML)
- IR SENSORS

WEAPONS LAB (AFWL)
- ELECTRONICS SHIELDING

ARMAMENTS LAB (AFATL)
- SWITCH FOR ELECTROMAGNETIC LAUNCHER

AFOSR
- CORE BASIC RESARCH PROGRAM
- SUPERCONDUCTING ELECTRONIC RESEARCH (SUPER) CONSORTIUM OF UNIVERSITY, INDUSTRY AND AIR FORCE LABORATORIES
- COORDINATION OF ALL AIR FORCE LAB EFFORTS IN HTSC

AIR FORCE APPLICATIONS OF SUPERCONDUCTIVITY+

FILMS (THICK OR THIN)

* ELECTROMAGNETIC SIGNAL DETECTION
(IR TO MM-WAVE RECEIVERS WITH IMPROVED SIGNAL-TO-NOISE AND COVERING THE OTHERWISE INACCESSIBLE SUBMILIMETER FREQUENCY RANGE)

* HIGH SPEED ANALOG AND DIGITAL ELECTRONICS
(UP TO 100X FASTER, UP TO 100X LESS POWER DISSIPATION AND MORE COMPACT, SAVING WEIGHT AND ENERGY)

* DIRECTED ENERGY WEAPONS
(SUPERCONDUCTING WALLS FOR RESONATING CAVITIES IN NEUTRAL PARTICLE BEAMS AND FREE ELECTRON LASERS WILL RESULT IN MORE POWERFUL BEAMS AND LIGHTER WEIGHT)

* MAGNETIC SHIELDING FOR MOTORS
(LEADING TO LIGHTER YET MORE POWERFUL MOTORS)

* EMP PROTECTION
(PERFECT REFLECTION OF EM WAVES WITH PASSIVE LIGHTWEIGHT SHIELDING)

* INERTIAL GUIDANCE UNIT
(SQUID-BASED UNITS CAN IMPROVE PRECISION BY UP TO 1,000X)

WIRE/TAPE

* ELECTROMAGNETIC LAUNCHERS
(GREATER THRUST AND HIGHER EFFICIENCY)

* ENERGY STORAGE
(UNIQUELY CAPABLE OF HIGH ENERGY DENSITY STORAGE AND NO LOSS WITH TIME)

* POWER SYSTEMS
(MORE POWERFUL AND MORE EFFICIENT MAGNETS)

* NATIONAL AEROSPACE PLANE
(MAGNETIC COOLING AND MHD WIND TUNNEL)

* POWER TRANSMISSION
(8 TO 15% SAVING IN ENERGY DUE TO LOSSLESS TRANSMISSION)

HIGH TEMPERATURE SUPERCONDUCTIVITY COULD IMPACT EVERY AIR FORCE SYSTEM THAT DEPENDS ON THE FLOW OF ELECTRIC CURRENT OR THE PRESENCE OF A MAGNETIC FIELD

+SECTIONS OF A SPACECRAFT CAN BE COOLED PASSIVELY TO 80K THROUGH THE USE OF SHIELDS AND RADIATORS. THUS, IT MAY BE POSSIBLE TO UTILIZE THE NEW HIGH TEMPERATURE SUPERCONDUCTORS ON SPACECRAFT WITH LITTLE OR NO ACTIVE REFRIGERATION

AIR FORCE SUPERCONDUCTIVITY TECHNICAL ACTION GROUP (STAG)
Revised 13 January 1988

Coordinator:	Dr. Harold Weinstock AFOSR/NE Bolling AFB DC 20332-6448	AV 297-4934
AFSC/DL	Mr. Tim Dues AFSC/DLAA Andrews AFB MD 20334	AV 858-3686

Lab POC's

AFAL	Capt. William Sowell, AFAL/CX *Dr. Claude Merrill, AFAL/MKPL Kirkland AFB NM 87117-6008	AV 525-5651 AV 525-5169
AFATL	Mr. Burt Colt, AFATL/XPX *Mr. William R. Thursby, AFATL/SAI Eglin AFB FL 32542-5434	AV 872-8591 AV 872-3160
AFGL	Capt. Vishnu Nevrekar, AFGL/LWG **Mr. William Hall, AFGL/XOP Hanscom AFB MA 01731-5000	AV 478-3486 AV 478-3606
AFSTC	Capt. Randy Unger, HQ AFSTC/XLD Kirtland AFB NM 87117-6008	AV 246-4960
AFWAL/PO	Dr. Charles E. Oberly, AFWAL/POOC-4 *1st Lt. Stephen R. Demers/AFWAL/POOC-4 Wright-Patterson AFB OH 45433-6543	AV 785-9185 AV 785-9185
AFWAL/AA	Mr. Stewart Cummins, AFWAL/AADA **Mr. Robert Remm, AFWAL/AAJ Wright-Patterson AFB OH 45433-6543	AV 785-8631 AV 785-4415
AFWAL/ML	Dr. Patrick Hemenger, AFWAL/MLPO **Mr. William Woody, AFWAL/ML Wright-Patterson AFB OH 45433-6533	AV 785-4474 AV 785-3561
AFWL	Mr. Darrell Spreen, AFWL/TA Kirtland AFB NM 87117-6008	AV 244-5828
RADC	Dr. Dallas T. Hayes, RADC/EEC *Dr. Nicholas F. Yannoni, RADC/ESE Hanscom AFB MA 01731-5000	AV 478-4264 AV 478-2206
	**Mr. B. M. Donovan, RADC/XPX Griffiss AFB NY 13441-5700	AV 587-3046

No Mark:	TPOC
*	Alternate TPOC
**	Program Manager

SUPERCONDUCTIVITY RESEARCH & DEVELOPMENT

AGENCY: **DEPARTMENT OF DEFENSE (NAVY)**

OFFICE: **OFFICE OF THE CHIEF OF NAVAL RESEARCH**
800 North Quincy Street, Arlington, VA 22217-5000

CONTACTS: Dr. Arthur M. Diness, Director, Engineering Sciences
Contract Research Department, Office of Naval Research
(202) 696-5074
(a) Dr. Robert C. Pohanka, Director, Materials Division
Engineering Sciences Directorate, ONR
(202) 696-4325
(b) Dr. Donald U. Gubser, Superintendent, Material Division
Naval Research Laboratory
(202) 767-2926

MISSION: Establish a technology base for Naval applications by addressing the frontiers of science with basic research and through a program of exploratory development which addresses Navy and Marine Corps technology needs.

OBJECTIVE: Exploit advances in superconductivity for Naval needs:
- o Understand & improve superconductor theory & properties.
- o Advance processing science to provide superconductors with optimized properties in technologically useful forms.
- o Advance design & fabrication techniques necessary for successful development of electronic, electrooptic, power and propulsion devices of interest to the U.S. Navy.

FUNDING: ($K) (L=Low Tc, H=High Tc)	FY87		FY88	
A. 6.1 Basic Research (ONR & NRL)	**(4124)**	**(1360)**	**(4205)**	**(1360)**
Physics	1100H	360L	1050H	350L
Electronics	620H	200L	415H	135L
Materials	1570H	525L	1700H	525L
Chemistry	370H	120L	640H	210L
Applied Sciences	465H	155L	400H	140L
B. 6.2 Exploratory Development	**(100)**	**(2565)**	**(500)**	**(2629)**
Electronic Devices	100H	300L	500H	300L
Mine Detection		1803L		1900L
Ship Propulsion		462L		429L
C. IR, IE, D at DNL Centers	**(756)**		**(1852)**	
NOSC Materials	359H		572H	
NWC Materials	69H		350H	
NSWC Materials	78H		300H	
NCSC Materials	250H		200H	
DTRC Materials			100H	
NADC Materials			330H	
D. Subtotals				
Basic Research	**5484**		**5565**	
Exploratory Development	**2665**		**3129**	
IR, IE, D at DNL Centers	**756**		**1852**	
E. Totals	8905		10546	

21JAN88

1. DoD - Strategic Defense Initiative

2. POC: Dr. Clyde J. M. Northrup, Jr.
SDIO/T/SL
The Pentagon
Washington, DC 20301-7100
(202) 693-1655

3. Mission: To develop critical High Temperature Superconductor (HTS) Components to respond to SDIO Element requirements in a time consistent with engineering technology insertion decisions.

4. R&D Objectives:

   a. To establish the information essential to evaluate the potential of HTS for critical SDIO applications.

   b. To develop and evaluate prototypes of enabling HTS devices, components and systems required by SDIO Elements.

5. FY87 Activities and Funding:

   a. Initial assessment of the HTS requirements: High Temperature Superconductivity; Technology, Applications & Prospects, NTIC.

   b. IR Detector development

   c. Electronics

   Funding $7.0M

6. FY88 Activities and Funding:

   a. High Current, High Energy Components: Conductors, Inductors, etc.

   b. Electronic/Photonic Devices: Detectors, Sensors, Electronic Components, Antennas, etc.

   c. RF Components: RF Cavities, Oscillators, Shields, etc.

   Funding: $34.0M

HIGH TEMPERATURE CERAMIC SUPERCONDUCTOR DEVELOPMENT

Agency: Defense Advanced Research Projects Agency

Office: 1400 Wilson Boulevard, Arlington, VA 22209

Contacts: Dr. Kay A. Rhyne, Program Manager of Ceramics and Superconductors,
also Dr. Ben A. Wilcox, Director of Materials Sciences Division
(202) 694-1303

Objective:

To advance the technology relating to processing, fabrication and demonstration of high temperature superconducting components.

Program:

$1.5 million of FY 87 funds were reprogrammed to fund ten unsolicited proposals (four universities, three industries and three Government laboratories). This work emphasized superconducting thin film and wire fabrication and phase diagrams.

A joint program with the Office of Naval Research has been undertaken with FY 88 funding which was in response to a Broad Agency Request for proposals. Two hundred three proposals were reviewed and 20 potential contractors have been selected. Contracts are expected to begin around June 1, 1988. Another 15 contracts which include Government laboratories and unsolicited proposals will be awarded. The program emphasizes components to be used in both civilian and military applications. These areas are:

1. Synthesis, processing and fabrication of materials in engineering shapes and sizes. Must optimize material properties for demonstration of component or device.
   - o Large scale: wires, fibers, filament tows, coils, tapes, monoliths, coatings and composites.
   - o Small scale: thin and thick film, patterning development.
2. Manufacturing issues of reproducible quality and quantity for military and commercial products.
   - o Quality control - identify key parameters that need control and develop means to do so.
   - o Address the state of present technology for scaling up laboratory HTSC Processes.

Large scale: powders, wires
Small scale: process control of interconnects/transmission lines and Josephson Junctions

3. Device design and demonstration.

   - Determine performance and cost improvement of HTSC devices in retrofitted systems.

   - Develop new system designs to take advantage of HTSC properties (different from lower temperature SC devices) or compensate for HTSC material limitations.

In later years, the program will include device development in the areas of A/D converters, analog devices, SQUIDS, digital computing, IR sensors, gyroscopes and accelerometers, motors, bearings, magnets and energy storage, RF cavities and antennas. A necessary part of this program is the material support efforts in both thin film and bulk superconductors. It is expected that this program will be expanded in the out years by DARPA and/or other agencies in the Government to include more applications and system developments particularly of revolutionary ideas that have not been possible before with lower temperature superconductors (e.g., semiconductor/superconductor hybrids and three terminal devices).

Funding Profile:

FY 87: $1.5 Million
FY 88: $20.4 Million
FY 89: $28 Million (desired)

Superconductivity Supported by NSF

- NSF support for basic research on superconducting materials will have increased to $16 or $17 million in FY 89 from about $6 million in FY 86 prior to the discovery of high temperature superconductors. Applications would add about $2.5 million per year. Total NSF support including applications may exceed $20 million in FY 1989 (see the attached table).

- Three distinct compounds with different chemical composition, structure, and critical temperatures have now been discovered which are superconducting at "high" temperatures.

  --"1-2-3" ($YBa_2Cu_3O_{7-x}$); T_c about 98K (Chu - February 1987)
  --Bismuth-aluminum-strontium-calcium-copper oxide; T_c about 114K (Chu - January 1988)
  --Thallium-barium-copper-oxygen (may be similar to 1-2-3); T_c about 81K

- The mechanism responsible for high temperature superconductivity is not yet understood. These different materials offer the possibility of observing common features which could provide insight into this mechanism.

- Condensed matter theorists have identified several competing models which will be tested by experiment. Good understanding could lead to the design of an improved superconductor.

- Research is being carried out on a broad front with NSF's support.

  --experiments on superconducting materials
  --theory of superconductivity
  --synthesis and processing of sc ceramics
  --testing in high magnetic fields
  --cooperative materials research efforts at MRLs involving interdisciplinary groups of physicists, chemists, materials scientists and engineers, comprising about $3 million per year
  --structure and properties of thin films

- Engineering support for quick-start grants ($50K for 6-12 months) with emphasis on processing.

  --sintering and hot extrusion; composites
  --polymer derived thin films and coatings
  --spray pyrolysis and chemical vapor deposition

- Initiative on development of superconducting generators, motors, and cables; endorsed by ENG Advisory Committee

  --planning group formed at May 26, 1987 meeting with EPRI, NBS, DOE, and several power companies
  --unsolicited pledges of $3 million from power companies
  --matched by $0.4 million from ECSE in FY 1988

May 1988

NSF SUPPORT FOR RESEARCH IN SUPERCONDUCTIVITY

($M)	FY86	FY87	FY88	FY89	(Estimates)
DMR					
Low Temperature Physics	1.8	2.3	2.6	2.9	
Solid State Physics	0.7	1.0	1.0	1.2	
Solid State Chemistry	0.1	0.2	0.2	0.2	
Condensed Matter Theory	0.8	1.0	1.8	1.8	
Total Condensed Matter Sciences	3.3	4.5	5.6	6.1	
Total Metallurgy, Polymers & Ceramics	0.5	0.6	0.9	1.4	
Materials Research Labs	0.0	2.0	3.5	4.0	
National Magnet Lab	1.5	2.0	2.5	2.5	
Instrumentation	0.4	0.6	0.1	0.6	
Division of Materials Research	5.7	9.6	12.6	14.5	
Division of Chemistry	0.1	0.3	0.5	0.8	
Division of Physics	2.2	2.4	2.5	2.5	
Engineering Directorate	0.1	1.3	1.9	2.2	
EPSCOR	0.0	0.1	0.1	0.1	
Total NSF	8.1	13.7	17.5	20.1	

ESTIMATES FOR HIGH Tc-RELATED RESEARCH AT NSF

($M)		FY86	FY87	FY88	FY89	(Estimates)
Div. Materials Research			8.0	10.0	12.0	
Chemistry Division			0.3	0.4	0.6	
Physics Division			2.2	2.3	2.4	
MSME	Engineering		0.9	0.7	0.7	
DMCE			0.1	0.4	0.5	
ECSE				0.4	0.5	
CBTE			0.3	0.4	0.5	
Total NSF			11.7	14.5	17.2	

STATUS REPORT ON NSF HIGH TEMPERATURE SUPERCONDUCTIVITY MATERIALS RESEARCH - JANUARY 1988

Prepared by D. Liebenberg, Program Director, Low Temperature Physics

Less than one year ago Professor P. Chu, with support from the Low Temperature Physics Program of the Division of Materials Research (DMR), announced the important discovery of superconductivity at above liquid nitrogen temperature. The prompt response of the scientific community to this significant scientific and technological challenge was simplified at the NSF where grantees have considerable flexibility to work on that research of most importance. Thus, in the DMR investigators in the Low Temperature Physics, Solid State Physics, Ceramics, Condensed Matter Theory Programs, and others, turned special talents and techniques toward this area of research. A new initiative was provided to the Materials Research Laboratories, where previously no thrusts in superconductivity had existed, to start three thrusts in the high temperature oxide superconductor materials. No additional funds have been provided to DMR in FY88 for support of research in high temperature oxide superconductors. Thus, the level of support remains at essentially the same level as at the end of FY87, having approximately doubled during that year as research interests changed. Total funding in the DMR is estimated at $8M.

A summary of other activities has been provided on the basis of incomplete returns from the several Programs at this early stage of the FY88. This report will emphasize the new research at the Materials Research Laboratories (MRLs). Thrusts were started at Stanford University, University of Illinois, and Cornell University, and thrusts are being started at MIT and Northwestern. Within the DMR budget there is planning to provide special funds for high temperature oxide superconductivity support; the details are still being fine tuned.

Stanford MRL - Structure and Properties of Thin Films

Approximately ten faculty intend to exploit what can be learned from thin films to understand the mechanisms of high temperature superconductivity. Films will be deposited in different manners, to exploit the particular advantages of each method to obtain special characteristics, and perhaps more importantly, to establish which properties are intrinsic and which result from the particular method of synthesis.

Progress - The recent discovery of high temperature superconductivity has dramatically changed the field of superconductivity. The reasons for the high Tc of these materials and the mechanism of superconductivity are unclear and this study to grow thin films has importance since films can be studied in a variety of ways, in particular, by tunneling spectroscopy. Thin films also have direct applications in electronic technology. At present the critical current of a polycrystalline film of $YBa_2Cu_3O_{7-x}$ grown on a $SrTiO_3$ substrate is greater than 10^4 A/cm^2 at 77 Kelvin, and order of magnitude higher than previously reported for bulk samples with random orientation and suggests it may not be necessary to produce single crystals or highly textured films in order to obtain usable current densities. Both point contact and sandwich-type tunnel junctions have been produced of both La-Sr-Cu-O (Tc = 40 K) and Y-Ba-Cu-O (Tc = 95 K). A consistent picture of very large gaps relative to the observed Tc and a multiple gap structure in differential conductance of these junctions have been observed. A multiple gap structure has been identified that may be a clue as to the superconductivity mechanism. The large magnitude of the gap will be significant for applications. Larger energy gaps imply better device performance in all regards. Project support is provided by NSF-MRL, ONR, and AFOSR.

University of Illinois - Mechanisms of High Temperature Superconductivity

This thrust focuses on the search for novel methods of preparation and processing of the new materials in thin film, bulk, and crystalline form, and on identification of the underlying mechanisms of superconductivity at high temperature. Some thirteen faculty are directly involved in this thrust. Close interaction among researchers concerned with materials processing, physical properties measurements, and superconductivity theory are hallmarks of this thrust effort. The possibility of achieving still higher transition temperatures is being pursued although not the central focus of this thrust.

Progress - Development of the sol-gel process of high-temperature superconductors has been made for both La-X-Cu-O and $YBa_2Cu_3O_{7-x}$ materials.

Northwestern University - High Temperature Superconductivity

The goals of this thrust group are to study the fundamental mechanisms of superconductivity at the high temperatures in the oxide materials and the development of the basic science required for fabrication and processing of these ceramic oxides. There are eleven faculty directly involved.

Theory, search of new materials, and fundamental properties experiments are currently underway. The transport and magnetization, magnetic resonance spectroscopies, conductivity and thermopower, and micro-structure are emphasized this year. Two techniques for preparation of the materials are physical and chemical vapor deposition.

Massachusetts Institute of Technology - Transition Metal Oxides

This new thrust area has two components. The first focuses on the fundamental scientific issues raised by the discovery of high temperature superconductivity. The second addresses the problems of processing high temperature superconducting materials. About ten faculty are directly involved in this thrust.

Progress - The group has been able to prepare single crystals large enough for neutron scattering experiments. An interesting soft mode driven structural phase transition has been observed. The processing part of the group has also successfully prepared superconductors with a transition of 90 K be oxidizing a suitable alloy.

Cornell University - Phase Boundaries and Novel Forms of Matter

Members of five of the six study groups have been active participants in an informal seminar to discuss recent Cornell results and reports from elsewhere. Preparation of bulk, single crystalline material, and thin films has been carried out, electrical and chemical characterization performed with electron microscopy and nmr measurements of the gap. New compounds have been explored. Theoretical work has been pursued along several lines. The MRL involvement has been primarily in the materials preparation facilities and the electron microscopy facility. Support for the several aspects of the program have come from a variety of sources.

Estimated support in the individual project Programs in the Division of Materials Research is summarized. The number of investigators counts each person involved independent of the extent, that is the number of grantees, and the funding level reflects an estimate of actual resources committed to the high temperature superconductivity research. The summary budget figures for the NSF are shown in the table.

Among the accomplishments is the growth by Prof. Ginsberg, University of Illinois, of single crystals with high homogeneity and dimensions in the millimeter range.

January 14, 1988
Revised 2/17/88

DESCRIPTION OF NSF ENGINEERING DIRECTORATE'S ACTIVITIES IN SUPERCONDUCTIVITY

1. Agency: NSF, Engineering Directorate

2. Contact: Dr. Shyam K. Samanta

3. Mission: The National Science Foundation through its Directorate for Engineering wants to rapidly promote advancement in processing capabilities of high T_c (>77K) superconducting materials in U.S. universities.

4. R&D Objectives: The high T_c superconducting materials are extremely brittle in bulk form, and innovative processing methods have to be developed to create stronger and more flexible forms of these materials for commercial applications. The objective of the NSF initiative is (1) to encourage researchers at U.S. universities with processing expertise to collaborate with the experts on synthesis and characterization of superconducting materials, and (2) to develop processing methodology of these materials into useful forms such as wires, rods, tubes, films, ribbons, etc.

5. FY 1987 Activities and Funding: In FY 1987 nineteen initiative grants were given to 18 universities, up to $50,000 per grant for periods of six months up to one year. The total funding for FY 1987 through this initiative was $857,199.

6. FY 1988 Plans and Estimated Funding: Nine grants have already been awarded in FY 1988, estimated at $500,000.

   Initiation grants would also provide a basis for follow-on proposals for FY 1988.

1/20/88

NATIONAL BUREAU OF STANDARDS

The National Bureau of Standards' research program has been developed to accelerate the progress of U.S. industry in commercializing superconductivity. NBS will develop and provide to U.S. industry measurement methods, verified materials data, required physical standards and measurement services which are important through all stages of research and development.

This program addresses the following four aspects of the technology associated with the new superconductors.

Materials Preparation and Characterization

The utilization of the high temperature ceramic superconductors will require the development of data which accurately describe materials characteristics required for processing. Data to be developed are focused on the development of phase diagrams necessary to ensure predicable processing as well as the chemical routes required for synthesis of homogeneous powders which can be processed at reduced temperature. This research is complemented by the development of techniques to allow compositional analysis for determination of elemental distribution and trace impurities.

Structure Determination

Characterization of the structure of superconducting materials is required for understanding the relationship of processing to properties. Techniques such as neutron detraction and field ion microscopy information to superconducting properties (e.g., Tc, Jc, magnetic properties). Particular emphasis will be placed on grain boundaries and their effect on superconductivity properties.

Property Measurements

The development of property measurement techniques is a critical portion of the program required for fundamental understanding as well as for application. Electron tunneling, x-ray fluorescence and photoelectron spectroscopy will be used to determine density of states and other fundamental properties.

Elastic properties determined acoustically will be used to study transformation and to infer Debye temperature. Optical and mass specrometric techniques will be developed to measure those properties necessary for thin film deposition.

Fabrication and Devices

Methods of making grain boundary microbridges, superconductor to normal junctions and Josephson junctions, as well as conductors, microstriplines, antennas, bolometers and SQUIDS operable at 77K will be investigated. Thin film deposition from multiple evaporative sources will be utilized. Special emphasis will be placed on application to metrology.

Funding - FY 1988 $2.8M
FY 1989 $9.3M (requested)

Contacts: NBS, Gaithersburg, S. J. Dapkunas, 301-975-6119
NBS, Boulder, CO; A. Clark, 303-497-3253

NASA-FUNDED SUPERCONDUCTIVITY
($M)

I. Low Temperature Superconductivity Applications

Office	Actual FY1987	Est. FY1988	Est. FY1989
OAST	0.6	0.9	1.3
OSSA			
o Gravity Probe B	1.0	1.0	1.0
o AXAF	0.5	0.75	0.75
TOTAL	2.1	2.65	3.05

Program Name/Description

Work includes low-temperature superconductivity device research utilizing superconducting-insulating-superconducting (SIS) junctions for remote sensing applications, superconducting quantum interference devices (SQUIDS) for magnetic sensing and gravity gradiometer instrument applications, superconducting cavities for precise time and frequency determination, and magnets for detector applications.

II. High-Temperature Superconductivity

Office	Actual FY1987	Est. FY1988	Est. FY1989
OAST	0.0	3.7	5.7
Office of Commercialization of Space	0.5	0.5	1.0
TOTAL	0.5	4.2	6.7

Program Name/Description

The high-temperature superconductor program includes benefit studies of typical aerospace applications. The research is directed at characterization of new high-temperature superconducting materials for unique aeronautics and space applications including the effects of vacuum and space radiation. Selected thin film and bulk device research will be supported for the most promising aerospace applications identified from the applications studies in data, communications, sensors, cryogenics, propulsion and power.

NATIONAL AERONAUTICS AND SPACE AGENCY
OFFICE OF AERONAUTICS AND SPACE TECHNOLOGY

The Office of Aeronautics and Space Technology (OAST) of National Aeronautics and Space Agency has instituted a program in High Temperature Superconductivity (HTSC) for applications in space and aeronautics. The program centers around five technology thrust areas:

I. Sensors
II. Cryogenics
III. Space Data Systems and Communications
IV. Space Power
V. Propulsion

I. SENSORS

This technology thrust area is complimentary to an ongoing base program in OAST in Senior Technology whose main focus is in the area of REMOTE sensing of galactic, planetary and terrestial phenomena. However the HTSC thrust covers this and insitu sensing applications for both space and aeronautics application areas. The tentative lead center in HTSC: Sensors in the Jet Propulsion Laboratory. The following is a partial list of areas of study, research and development in this thrust area with NASA centers of interest:

Accelerometers for spaced and aeronautics applications, Goddard Space Flight Center and the Langley Research Center.

Magnetometers, Jet Propulsion Laboratory.

Gyroscopes, Marshall Space Flight Center.

Superconducting/Insulating/superconducting junction devices for heterodyne mixer coherent detection systems and non-linear frequency applications, Jet Propulsion Laboratory and the Langley Research Center.

Local oscillators for heterodyne coherent detection systems, Jet Propulsion Laboratory.

HTSC bolometers fabricated in arrays for non-coherent detection systems, Marshall Space Flight Center and the Goddard Space Flight Center.

Field induced gap detectors for non-coherent detection systems, Jet Propulsion Laboratory.

HTSC components for Synthetic Aperature Radar detection systems, Jet Propulsion Laboratory.

Anemometer detectors for wind tunnel applications, Langley Research Center.

Technology transfer to aeronautical applications is expected.

II. CRYOGENICS

This technology thrust area is also complimentary to an on-going base program in OAST in Cryogenic Technology which is part of the base Sensor Technology program. Cryogenic technology which revolves around the cooling of focal planes for the various remote observations instruments is key to future NASA missions, such as the Large Deployable Reflector, a 10 to 15 meter reflector for observations from the submillimeter to far infra-red portions of the electromagnetic spectrum. The following is a partial list of areas of study, research and development in this thrust area:

Ground planes. Instruments such as focal plane detector arrays and associated electronics need an adequate ground plane for noise reduction, Goddard Space Flight Center.

Thermal Isolation, Goddard Space Flight Center.

Magnetic Bearings, Goddard Space Flight Center, Lewis Research Center, Langley Research Center, Marshall Space Flight Center.

Magnetic Refrigeration, Goddard Space Flight Center.

Instrument/Component Electronics interface, Langley Research Center.

III. SPACE DATA SYSTEMS AND COMMUNICATIONS

This technology thrust area is complimentary to an on-going base program in OAST in Space Data and Communications Technology. Space data technology is key to the on-board processing of space generated data, which can exceed estimates of a gigabit per second. This massive computational rate is key to future NASA missions, such as the Mars Rover and Sample Return Mission, where some degree of autonomy is a critical mission requirement. In addition, space-based communication is critically important for both geostationary-to-geostationary, geostationary-to-low-earth-orbit and deep-space-to-geostationary missions. High speed microwave components and ultra-stable oscillators are critical to enable these missions. The following is a partial list of areas of study, research and development in this thrust area.

High speed switches, Jet Propulsion Laboratory.

Ultra stable oscillators, Jet Propulsion Laboratory and the Goddard Space Flight Center.

Microwave components, Jet Propulsion Laboratory and the Lewis Research Center.

Low noise receivers, Jet Propulsion Laboratory.

Signal distribution, Jet Propulsion Laboratory and the Lewis Research Center.

IV. SPACE POWER

This technology thrust area is complimentary to an ongoing base program in OAST in Space Power Technology. Space power technology is key to future NASA missions, such as the Mars Rover and Sample Return Mission, where the mission science requirements is heavily dependent on available power and storage. Concepts and technologies in this area have obvious spin-offs in the aeronautics area.

Space power storage, Lewis Research Center.

Space power distribution and transmission, Lewis Research Center.

Large scale shielding, Lewis Research Center and the Langley Research Center.

Vibration isolation, Lewis Research Center and the Langley Research Center.

Hypersonic braking, Lewis Research Center.

Eddy current dampers, Lewis Research Center.

Levitation, Lewis Research Center and the Langley Research Center.

V. PROPULSION

This technology thrust area is complimentary to an ongoing base program in OAST in Propulsion Technology. Propulsion technology is key to future NASA missions. The proposed manned mission to Mars and the return to the Moon for a permanent manned station are just a few areas where this technology is a prerequisite. Included are a host of applications technologies also devoted to the aeronautics area.

Electromagnetic vehicle launchers, Lewis Research Center.

Magnetoplasmadynamic thruster, Lewis Research Center.

Magnetic bearings, Lewis Research Center and the Langley Research Center.

Levitation/aircraft, Lewis Research Center and the Langley Research Center.

Drive system/rotorcraft, Lewis Research Center and the Langley Research Center.

Orbit relocation, Lewis Research Center.

Aircraft boundry layer control, Lewis Research Center and the Langley Research Center.

Aircraft controls, Lewis Research Center and the Langley Research Center.

Electric propulsion, Lewis Research Center and the Langley Research Center.

Contact:

Dr. Martin M. Sokoloski
Information Sciences & Human Factors
Division/RC
Office of Aeronautics and Space Technology
NASA
Washington, DC 20546
(202) 453-2748

1. Department of the Interior/Bureau of Mines/Minerals and Materials Science

2. Murray A. Schwartz (634-1865)

3. Mission: Research to further mining, minerals, and materials technologies while helping to conserve our Nation's resources.

4. R&D Objectives: To fabricate high-temperature ceramic superconductors for such uses as electrical transmission, motors, and magnets.

5. FY 87 Activities and Funding: Pioneering (less than $100 K) effort on studying the fabrication of ceramic superconductors.

6. FY 88 Plans and Estimated Funding: Pioneering (approximately $100 K) efforts are continuing on fabrication studies, particularly on studying the effect of ozone atmosphere sintering as compared to oxygen sintering. Other fabrication studies on hot-pressing and hot-extrusions are planned at our Tuscaloosa (AL) Research Center. Three pioneering efforts recently started at our Albany (OR) Research Center are on forming ceramic superconductors by alloy oxidation, on replacing some of the oxygen with sulfur, and on thermodynamic measurements. The Bureau is co-sponsoring a "National Conference on the First Two Years of Ceramic Superconductivity" with the University of Alabama-Tuscaloosa on April 11-13, 1988. Attendance will be limited to 150 people with no more than two per organization.

High Temperature Superconductivity R&D

Agency/Office: DHHS/PHS/National Institutes of Health

Contacts: Dr. Philip S. Chen, Jr.
Associate Director for Intramural Affairs
National Institutes of Health
Building 1, Room 103
Bethesda, MD 20892
(301) 496-3561

Mission: To improve health of the Nation by increasing our understanding of the processes underlying human health and by acquiring new knowledge to help prevent, detect, diagnose, and treat disease.

R&D Objectives:

- o Supporting research in universities, medical schools, hospitals, and research institutions in this country and abroad;
- o Conducting research in its own laboratories and clinics;
- o Supporting training for promising young researchers;
- o Helping to develop and maintain research resources;
- o Identifying research findings which can be applied to the care of patients, and helping to transfer such advances to the health care system;
- o Promoting effective ways to communicate biomedical information to scientists, health practitioners, and the public; and
- o Developing and recommending polices related to the conduct and support of biomedical research.

FY 1987 Activities and Funding:

NIH did not support directly any research or development projects on high temperature superconductivity. Several types of instruments utilizing superconducting magnets are used in biomedical research, including nuclear magnetic resonance (NMR) spectrometers and scanners, and superconducting quantum-interference device (SQUID) susceptometers.

FY 1988 Plans and Estimated Funding:

NIH does not plan any direct support of research projects on high temperature superconductivity.

1. AGENCY/OFFICE: Department of Transportation

2. CONTACTS: George Kovatch
Transportation Systems Center
Kendall Square
Cambridge, MA 02142
617-494-2756

Vincent R. DeMarco
Urban Mass Transportation Administration
URT-20
400 7th Street, S.W.
Washington, DC 20590
202-366-0224

Shang Q. Hsiung
Urban Mass Transporation Adminstration
URT-22
400 7th Street, S.W.
Washington, DC 20590
202-366-0222

3. MISSION: Monitor developments in superconductivity through technical newsletters, reports, studies, attendance of conferences, meetings, etc.

4. R&D OBJECTIVES: Evaluate developments in superconductivity for potential applications in the transportation sector.

5. FY 1987 ACTIVITIES AND FUNDING: SBIR funding for $50,000 for SQUID weapons detection instrument.

6. FY 1988 PLANS AND ESTIMATED FUNDING: The Commonwealth of Pennsylvania plans to activate the Boeing MAGLEV technology at a transportation facility in Pennsylvania where this technology will be analyzed for its market potential. Estimated funding of $255,000.

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D.C. 20506

CHARTER OF THE SUPERCONDUCTING MATERIALS SUBCOMMITTEE OF COMAT

June 9, 1987

ESTABLISHMENT:

In accordance with its charter, the Committee on Materials (COMAT), which is a committee established by the Federal Coordinating Council for Science, Engineering, and Technology (FCCSET), hereby establishes a Superconducting Materials Subcommittee (SMS) as a means to accomplish its responsibilities in that area.

LEGISLATION:

FCCSET was established by P.L. 94-282, "National Science and Technology Policy, Organization and Priorities Act of 1976". COMAT constitutes the primary means through which the Office of Science and Technology Policy carries out the provisions of P.L. 96-479, "National Materials and Minerals Policy, Research and Development Act of 1980", of P.L. 98-373, "National Critical Materials Act of 1984", and other responsibilities indicated in the President's 1982 "National Materials and Minerals Program Plan and Report to Congress".

PURPOSE:

The purpose of the SMS is to coordinate superconducting materials research among the agencies of the federal government, to identify relevant issues and concerns, and to bring them to the attention of COMAT.

The SMS should focus its attention on long-term, high-risk, basic research in superconducting materials that has the best chance for wide generic application and can play an important role in U.S. global competitiveness.

ADMINISTRATIVE PROVISIONS:

(a) The SMS shall report to the Chairperson of COMAT, providing information on its activities. The Chairperson of SMS shall be appointed by the Chairperson of COMAT, for a tenure to be determined by the COMAT Chairperson.

(b) Meetings of the SMS shall be called by the SMS Chairperson as deemed appropriate. Each agency member shall be responsible to attend or have an appropriate alternate attend.

(c) The Chairperson of SMS may initiate ad hoc working groups or task forces to undertake studies, analyses, or interagency coordination in the field of superconducting materials with participation not restricted to COMAT members.

(d) Agencies to be represented on SMS shall be selected by COMAT. Members shall be nominated by their respective agencies subject to approval of the SMS Chairperson and COMAT. The members should be program directors and other representatives who are qualified to address scientific and technological aspects of superconducting materials.

(e) The SMS shall continue as described herein unless terminated by COMAT.

Louis Ianniello

Louis Ianniello
Chairperson, SMS

G. W. Henderson

Gregory W. Henderson
Chairperson, COMAT

MEMBERSHIP LISTING

OSTP

Dr. William Owczarski
Chairman, COMAT
Office of Science & Technology Policy
Executive Office of the President
Washington, DC 20506

Dr. Tom Gillespie
Office of Science & Technology Policy
Executive Office of the President
Washington, DC 20506

Dr. Darrell H. Reneker
Executive Secretary, COMAT
Office of Science & Technology Policy
Executive Office of the President
Washington, DC 20506

Dept. of State

Dr. Charles M. Newstead
Senior Advisor for Energy Technology
Department of State
Room 7820
Washington, DC 20520

Bureau of Mines/DOI

Dr. Murray A. Schwartz
Program Manager for Materials
Dept. of Interior/Bureau of Mines
2401 E Street, N.W.
Washington, DC 20241

Mr. James B. Hedrick
Rare Earth Commodity Specialist
Dept. of Interior/Bureau of Mines
2401 E Street, N.W.
Washington, DC 20241

Dr. Charles A. Sorrell
Chief, Office of Advanced Materials
Coordination
Dept. of Interior/Bureau of Mines
MS 5201, Room 968
2401 E Street, N.W.
Washington, DC 20241

DOD

Dr. Ted G. Berlincourt
Director of Research & Laboratory Management
Department of Defense
Room 3E 114, Pentagon
Washington, DC 20301

Navy

Dr. Arthur M. Diness
Assoc. Director for Research (Eng. Sci)
Office of Naval Research
800 North Quincy Street
Arlington, VA 22217

Mr. Charles Craig
Office of Naval Research
800 North Quincy Street
Arlington, VA 22217

Dr. Robert C. Pohanka
Director, Materials Division
Office of Naval Research
800 North Quincy Street
Arlington, VA 22217

Dr. Ken Davis
Office of Naval Research
800 North Quincy Street
Arlington, VA 22217

Dr. Don Liebenberg
Office of Naval Research
800 North Quincy Street
Arlington, VA 22217

Army

Dr. Gerry Iafrate
U.S. Army Electronic Technology and Devices Laboratory (LABCOM)
SLCET-E
Fort Monmouth, NJ 07703

Air Force

Dr. Donald R. Ulrich
Program Manager
Chemical Structures - Ceramics Glass and Polymers
Air Force Office of Scientific Research/NC
Bolling Air Force Base, Bldg. 410
Washington, DC 20332

Dr. Harold Weinstock
Program Manager, Superconductivity
Air Force Office of Scientific
Research/NE
Bolling Air Force Base
Washington, DC 20332

Dr. John O. Dimmock
Technical Director
Air Force Office of Scientific
Research
Bolling Air Force Base, Bldg. 410
Washington, DC 20332

SDI

Dr. Clyde Northrup
Special Assistant
Materials and Structures
Strategic Defense Initiative Organization
The Pentagon
Washington, DC 20301-7100

DARPA

Dr. Kay Rhyne
Program Manager
Ceramics and Superconductors
DARPA/DSO/MSD
1400 Wilson Boulevard
Arlington, VA 22209-2308

Dr. Benjamin A. Wilcox
Assistant Director
Materials Science
DARPA/DSO/MSD
1400 Wilson Boulevard
Arlington, VA 22209-2308

NSF

Dr. William T. Oosterhuis
Acting Section Head, Special Programs
in Materials
Division of Materials Research
National Science Foundation
1800 G Street, N.W.
Washington, DC 20550

Dr. Win Aung
Division Director
Division of Mechanics, Structures,
  and Materials Engineering
National Science Foundation
1800 G Street, N.W.
Washington, DC 20550

Dr. Shyam K. Samanta
Program Director
Materials Engineering and
  Processing Program
National Science Foundation
1800 G Street, N.W.
Washington, DC 20550

NASA

Dr. Martin Sokoloski
NASA
OAST/RC
600 Independence Avenue, S.W.
Washington, DC 20546

Dr. Daniel R. Mulville
Program Manager for Structures
NASA, OAST/RM
600 Independence Avenue, S.W.
Washington, DC 20546

DOC/NBS

Mr. Stanley J. Dapkunas
Deputy Chief, Ceramics Division
National Bureau of Standards
Building 223, Room A249
Gaithersburg, MD 20899

NIH

Dr. Philip S. Chen, Jr.
Associate Director for Intramural Affairs
National Institutes of Health
Building 1, Room 103
Bethesda, MD 20892

DOT

Dr. George Kovatch
Chief, Office of University Research
  and Technical Innovation
Department of Transportation, DTS-23
Kendall Square
Cambridge, MA 02142

Dr. Tony Yen
Associate Administrator
Research and Technical Assistance
Urban Mass Transportation Admin. (DOT)
URT-1, Room G413
400 7th Street, S.W.
Washington, DC 20590

Mr. Vincent R. DeMarco
Acting Director, Office of Bus and Paratransit Systems
Urban Mass Transportation Admin. (DOT)
URT-20
400 7th Street, S.W.
Washington, DC 20590

Mr. Shang Hsiung
Urban Mass Transportation Admin. (DOT)
URT-20
400 7th Street, S.W.
Washington, DC 20590

National Critical Materials Council

Mr. Robert Wilson
Executive Director, White House Critical Materials Council
MS 1100, Room W725, Columbia Plaza
2401 E Street, N.W.
Washington, DC 20241

OTA

Mr. Wendell Fletcher
Office of Technology Assessment
U.S. Congress
ITE Program
Washington, DC 20510-8025

DOE

Dr. Louis Ianniello
Deputy Associate Director for Basic Energy Sciences, ER-11
U.S. Department of Energy
Washington, DC 20545

Dr. Iran Thomas
Director
Materials Sciences Division
Office of Basic Energy Sciences, ER-13
U.S. Department of Energy
Washington, DC 20545

Mr. Kenneth W. Klein
Director of Energy Storage and
  Distribution, CE-32
U.S. Department of Energy
Washington, DC 20585